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TECHNICAL COMMUNICATION No. 2

FIELD EXPERIMENTS IN HORTICULTURE

by

T. N. HOBLYN, DIP. HORT. (Wye),
Statistician, East Malling Research Station, Kent.

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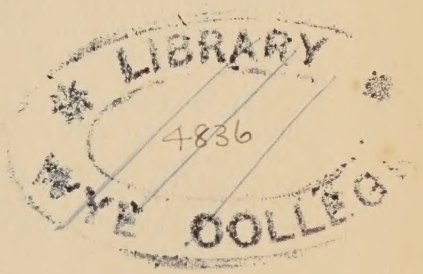

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Since the opening of the Bureau of Fruit Production many enquiries have been received on various subjects, but none more numerous than those on the lay-out of horticultural experiments in the field.

A session was devoted to the subject at the Imperial Horticultural Conference, held in London, in August, 1930, when the need was expressed in more than one quarter for an exposition of the ways in which Dr. R. A. Fisher's methods, described by him and Dr. J. Wishart, in Technical Communication No. 10* of the Imperial Bureau of Soil Science, might best be adapted to the difficulties peculiar to horticultural experiments.

It was therefore decided to collect the data available on horticultural field experiments in the light of modern research and to issue these in a memorandum.

The opportunity was taken of getting the views of many directors of horticultural experiments in the Tropics then present in this country. They responded most readily, explaining the particular difficulties with which they are faced, and making suggestions as to how best these may be overcome.

Mr. T. N. Hoblyn, who has of recent years been applying statistical method to the layout of fruit experiments at the East Malling Research Station, was asked to prepare a memorandum on the subject.

It is hoped that horticulturists reading it will send in their criticisms of the methods suggested and details of any experiments bearing on the many points raised.

* The arrangement of field experiments and the statistical reduction of the results.

FIELD EXPERIMENTS IN HORTICULTURE.

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FOREWORD.

The large amount of experience gained during the last few years, both in this country and abroad, in the use of field experiments has at the present time resulted in the apprehension of definite and clearly defined principles as to the manner in which such experiments should be carried out. This experience is of two kinds : (a) that obtained incidentally by carrying out field trials by various methods, and (b) that obtained from deliberate experiments such as uniformity trials, designed to discover the most satisfactory methods of experimentation. Both lines of evidence have yielded under statistical examination concordant evidence of the importance and general applicability of the three principles which govern modern experimental design.

(i) Replication.

That field trials, like nearly all other biological experiments, should be conducted in replicaet is probably universally acknowledged, and the necessity for replication in field trials has been particularly forced upon the attention of experimenters by the great variability of the soil factor. In early years it was not, however, so clearly apprehended that replication serves two distinct purposes. On the one hand it serves to diminish the standard error of the experimental comparisons, or in other words it increases their precision ; for this purpose it is not, however, the only means available, but may with advantage be supplemented by increased care in obtaining uniformity of land, material and method of treatment. On the other hand, it provides, what is essential if the experiment is to have any scientific reliability, an estimate of error. This latter purpose can be obtained by no other means save by replication, and it has been an obstacle to the development of adequate methods, that while the first and, in a sense, unessential purpose of replication was easily recognised, its second and essential purpose has often been overlooked and consequently sacrificed.

(ii) Randomization.

Misapprehension as to the essential purpose of replication led in the past to some carelessness as to whether the experiment was or was not capable of supplying a valid estimate of error. It is manifestly impossible to compare different treatments on identically the same land in the same year, and the experimental method is essentially to compare the results of different treatments on different samples of the same land. This method is certainly adequate if the samples are in fact random samples of plots of the experimental area, but is without theoretical justification when the treatments are allocated to systematically arranged plots, on the assumption that soil heterogeneity is in fact unrelated to the system of arrangement chosen. Consequently, in modern arrangements an element of randomization is always introduced, this being of such a kind as to ensure that the differences utilised in the estimation of error are properly representative of the other differences which produce the actual errors of the experiment.

(iii) Localization of Control.

It has frequently been alleged that increased replication, by increasing the area of ground required, may fail to produce a proportional increase in precision. This difficulty is completely overcome by confining the comparisons from which both the errors and their estimates are derived to limited areas of land, which are called Blocks, Rows, Columns, etc., according to the form of the experiment.

The experiment is planned so that all soil differences between such areas are eliminated by the field arrangement from the comparisons to be made, and, by the Analysis of Variance applied to the results, are eliminated equally from the estimate of error. The localization of control often greatly enhances the precision of the experimental results, being in this respect equivalent to the more expensive process of increasing the replication, and it is on the possibility of reconciling this desideratum with the requirement that the estimate of error should be a valid one, that modern methods of field experimentation are built up.

In the application of these principles it is most undesirable to lay down any one system of arrangement as universally the best. A wide variety of possibilities is open to the experimenter without violating the requirements of valid experimentation. Every type of crop should in this respect be treated on its merits, since only by practical experience in experimentation with like material can one be sure of adopting the most economical, or the most productive types of field arrangement. It is, I believe, as a step towards the development of adequate methods in horticultural experimentation that the value of Mr. Hoblyn's report will be found to lie.

ROTHAMSTED EXPERIMENTAL STATION,
HARPENDEN.

R. A. FISHER.

FIELD EXPERIMENTS IN HORTICULTURE.

INTRODUCTION.

The art of field experimentation has made such rapid advances during the last decade as to give to the research worker an entirely new outlook on the possibilities for agricultural and horticultural research in the field. New methods have been introduced which promise, and have indeed achieved, a degree of precision previously undreamt of. That the world of horticultural science has been perhaps rather slower to adopt these methods is somewhat natural. Fruit trees, not being annuals, present much greater initial difficulties than those confronting the agriculturalist.

Horticultural material takes years to prepare and many more years to mature, and an experiment already in being cannot lightly be abandoned in order to adopt a new method as yet unproven, even though that experiment may not be giving the best results.

Even so it has been obvious for some time that something has been amiss, not only with the methods of field experiment used in horticulture, but also with the material available for experiment. The purpose of this present paper is :—

(1) To consider how this material has been improved in recent years.

(2) To review the methods of field experimentation which have been used during the last thirty years and are being used to-day. Examples will be given showing the lay-out and management of three modern types of experiment and an analysis of the results made.

(3) To discuss the application of modern methods of experimentation to crops where the improvement in the material has been only partial or is of unproved value.

The examples given all refer to deciduous fruits. It is felt, however, that the methods both of lay-out and analysis of results can in most cases be readily adapted to tropical crops, and accordingly the particular difficulties met with by workers in the tropics and sub-tropics are also considered briefly in this paper.

I. EARLY HORTICULTURAL EXPERIMENTS AND CAUSES OF THE FAILURE THEREOF.

When Bedford and Pickering founded the Woburn Experiment Station for the elucidation of horticultural problems, little was known of the art of field experiment and there had been no horticultural experience at all. Pickering therefore followed the example of the Rothamsted experimenters and divided his land into a series of plots, in each of which he planted, instead of a cereal crop, a certain number of fruit trees or bushes. Some of these plots would be untreated and the others would each be subjected to a different treatment. Thus a plantation of 60 trees of Bramley's Seedling apple might be divided up into ten plots each containing six trees. Three of these plots might be untreated, while each of the other seven would receive a different manurial treatment. Such a trial was repeated using several varieties of apples, but no plot combining the same variety and manurial treatment would occur more than once.

Pickering's problem, however, differed from that at Rothamsted, for, while Lawes and Gilbert were content with the bulk yields from a single plot comprising a large number of individual plants, knowing that they could go on repeating this annually until their results were established, Pickering could not repeat his plantings indefinitely year by year and so was forced to observe the behaviour of each of the individuals within his plot over a period of years, in order to ascertain whether the average for that plot really represented the performance of the trees in it (14 and 85). In addition he found that it was useless to rely for his information on the records of mere crop, and found that such manifestations of vigour as girth of stem, length of new shoots formed and stoutness of shoots, were all of the first importance in interpreting the tree's performance. This materially added to the labour involved.

The Woburn method was extensively copied, and American literature in particular is full of accounts of what Cooper calls the " old familiar plot series fertilizer experiments." (29).

These earlier workers, however, soon found that experiments with fruit trees " exhibit errors from sources which do not generally affect experiments in agriculture " (85), and it soon became apparent that both the material which they used and their methods of experimentation would need great improvement, if such experiments were to be of any practical value. Thus Chandler, one of the foremost of American workers, emphasizes the fact that such field experiments " have

not yielded as valuable results as was expected of them." This he attributes to the high variation and notes that " experimental results have been published in which the yield of the treated plots was nearly twice that of the untreated plots and yet it was not certain that the difference was due to the treatment " (19).

Workers at the Geneva and Pennsylvania Stations also deplore the meagreness of the results of a period covering some 30 years of fertilizer experiments (63) (24) (3).

Why have these early trials failed to yield information and what are the causes of these large errors to which they were subject?

Field experiments are subject to error from two main sources : (i) the inherent variation in the material which is being dealt with, and (ii) the variation which is due to outside factors reacting on the plant after the experiment has been planted up.

(i) The inherent variation in the trees themselves.

The inherent variability in a set of fruit trees is primarily determined by the way in which the tree is built up. Three causes are mainly responsible for the very variable material with which the earlier workers had to deal.

(*a*) *The use of seedling varieties.*

Since practically no fruit tree can reproduce itself exactly from seed, it stands to reason that varieties so raised will always be very variable. With the exception of walnuts and certain tropical fruits, the use of trees raised in this way is now becoming rarer in horticulture, but where the method is still in use, the variation is so great that such material is of little value for any but the crudest experiments, which are at best a laborious and uncertain undertaking.

Thus Batchelor and Reed give the results of 2 years crop records on a 24 year old seedling walnut grove comprising 280 trees, which show the high coefficients of variability* of 48% and 54% for the two consecutive years (9).

Similarly Jack notes the high variation in coconuts (73), and Heusser gives an account of the efforts to improve rubber plantations raised in this way (66).

(*b*) *The use of clonal varieties carelessly raised.*

Most of the small fruits of this country, such as raspberries and currants, were originally raised from a single plant and propagated by vegetative means.

Two causes have been responsible for variation in such material : (i) the presence of rogues in the nursery beds which were propagated together with the original clone ; (ii) the taking of cuttings, etc. from diseased plants.

Hatton (55) and Grubb and Peren (50) give accounts of the classification work necessary before trials could be commenced with black currants and raspberries, which were badly mixed owing to the first cause ; while the propagation of black currants from bushes affected with the disease known as " reversion " may be cited as an example of the havoc in such plantations due to the second.

(*c*) *The use of clonal varieties budded or grafted on to seedling rootstocks.*

The building up of a tree from two individuals, the rootstock and the scion, is a very ancient practice and is the most usual method of propagating deciduous fruit trees, citrus, mangoes and more recently certain other tropical fruit trees. An exhaustive account of the method has been recently given by Hatton (61).

But while there is little evidence of inherent variation due to bud mutations, etc. in the scions, which are the vegetative progeny of clonal varieties (33) (99) (87) (104) with the exception

---

***The Coefficient of Variability.**

This statistic has been widely used by horticultural writers to express the degree of variability of their material. It is, therefore, used here for comparative purposes. It is simply the standard deviation as a percentage of the mean, and like the mean its reliability is dependent on the way in which it is obtained. Thus if it is found from a few observations only, it is less reliable than where the numbers are large. As the coefficient itself increases, the dependence which can be placed upon it decreases. Appendix A gives a table showing the standard error of the coefficient of variability for various values of itself and different numbers of observations. This standard error may be used in exactly the same way as the standard error of the mean to find out whether one series of observations is really more variable than another.

of certain classes of citrus fruit (101), the seedling rootstock has been justly blamed as one of the chief causes of the enormous variation with which the workers of a generation ago had to contend.

Numerous examples of such variation can be found in horticultural literature, though it is difficult to find many cases in which a direct comparison between trees on seedling and standardized rootstocks is possible.

At East Malling a few such comparisons are possible ; for example on one plot devoted to an apple pruning trial 5 varieties are grown on both seedling crab and commercial paradise rootstocks, and in every case the trees on crab are more variable ; indeed, it is possible to find amongst eight trees in one row of Early Victoria on crab two trees, one of which has in 14 years produced 10 times the crop of the other. The difficulty of trying to show a 50% increase due to manurial, or other treatment on such material is obvious. A plantation of young half-standard Lord Derby apples may also be cited. Here 10 trees on Siberian Crab seedlings had a coefficient of variability of 22% for cross section of stem, while two exactly comparable sets of 12 trees on clone rootstocks had coefficients of variability of only 7% and 6.6% for the same character.

Most of the evidence on variability to be found refers to the cropping of mature trees in one or more years and is thus difficult of comparison, but in nearly all cases given the coefficients are between 30% and 40% of the mean and in some cases over 50%. Space does not permit of a detailed presentation of the experiences of other workers ; but such pomologists as Sax and Gowen (97), Hedrick and Anthony (62), Dorsey and Knowlton (35) and Anthony and Waring (3) all agree that the seedling rootstock is the cause of much of the variation in their apple orchards, while Batchelor and Reed were reduced to the " not entirely satisfactory " expedient of discarding abnormal trees and substituting the mean of the eight surrounding trees and were even then only able to reduce the coefficient of variability of the yield of 224 Jonathan apple trees to 41.2% (9).*

The same state of affairs has existed in the pear, plum and cherry orchards of this country, complicated by the fact that some seedlings display incompatibility with certain varieties, and Webber notes the probability of seedling rootstocks being responsible for the high variation in citrus groves (113).

Further support for the contention that the seedling rootstock really is responsible for much of this high variation will be found in a succeeding section of this paper, when the behaviour of trees on standardized stocks will be discussed.

(ii) The variation due to outside causes.

(*a*) *Positional variation.*

Second only to rootstock influence, differences in position on the large areas of ground covered by horticultural experiments have been the most widely blamed source of error. Such influences as differences in soil fertility, wind, and sometimes disease show themselves in this way.

Figs. (i) and (ii) show the influence of position on the variation of two large experiments. In the first case some 5 acres of Baldwin black currants at East Malling were harvested in 36 plots, each containing 210 bushes. In these large plots there is little chance of individual bush variation showing itself and the differences in crop shown in fig. (i) are undoubtedly due to some outside factor, in this particular case probably shelter, since the low yielding plots at the north end are in a very exposed position. If the amount of variation contributed to the whole by this factor is estimated and eliminated, as is usually done in the analysis of the records, the coefficient of variability in this case is reduced from 16.4% to 10.7%.

Fig. (ii) shows the variation in crop which can be attributed to position in Batchelor and Reed's plantation of 1000 navel oranges at Arlington, California. Here the plantation is divided into 100 plots each containing 10 trees and the low and high yielding parts of the field are obvious. By eliminating the positional variation here, the coefficient of variability for plots of 10 trees is reduced from 28.2% to 14.9% (9).

---

* Reference to Appendix B shows that with a coefficient of variability of 40% each similarly treated tree would have to be repeated 32 times in order to prove a difference of 20%. This gives an idea of the prohibitive size which experiments with such material would soon attain if they were to be of any value.

FIG. I. Variations due to position in a plantation of Baldwin Black Currants at East Malling.

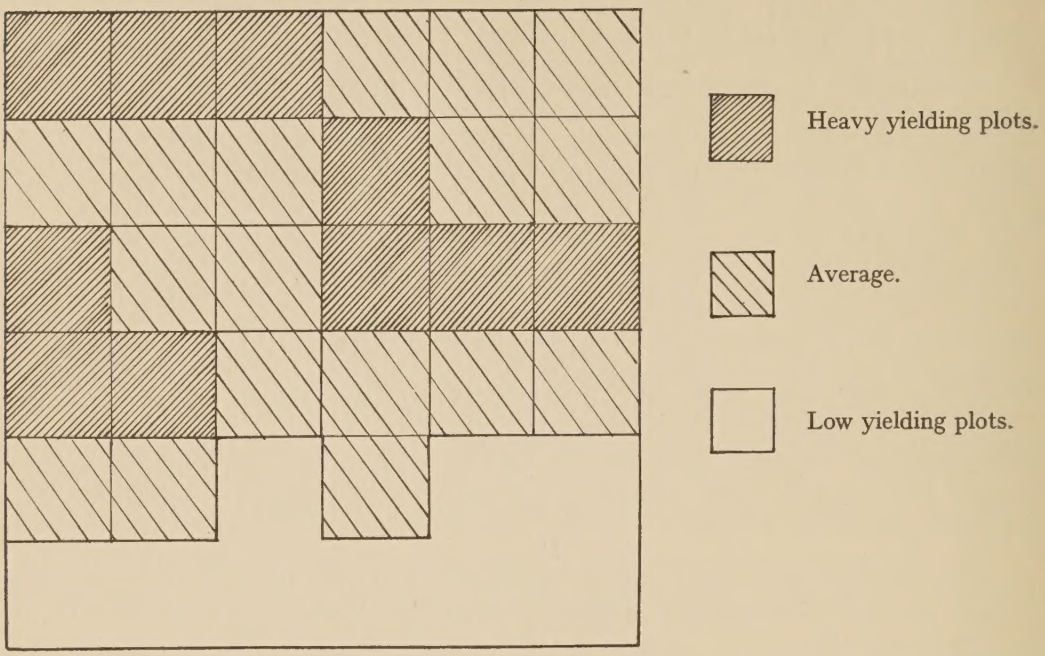
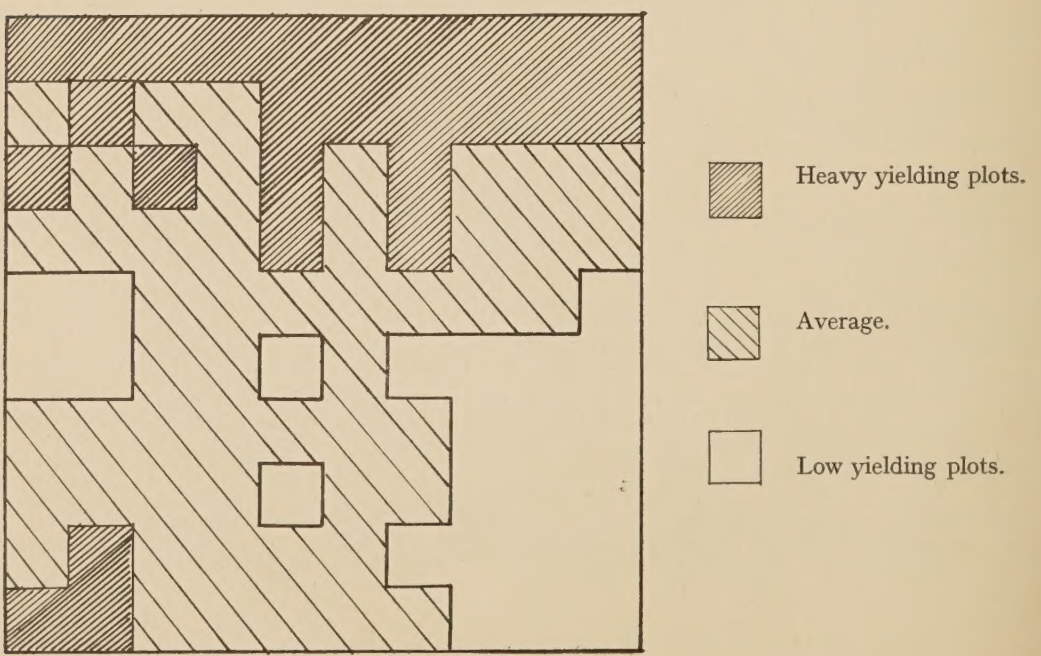


FIG. II. Soil variation in a Navel Orange Grove. (Batchelor & Reed's data).



One further example may be quoted of the effect of soil difference on apple trees on a known clonal rootstock at East Malling. Here eight trees of Lane's Prince Albert on Jaune de Metz in one part of the field gave an average of 63 lbs. of fruit per tree, while another eight trees, identical except in position on the ground, averaged 90 lbs. per tree, a difference which is statistically significant. That this difference is really due to variation in soil fertility and not to any other cause is in this case very clear owing to the unmistakably characteristic growth and cropping of trees on this rootstock.

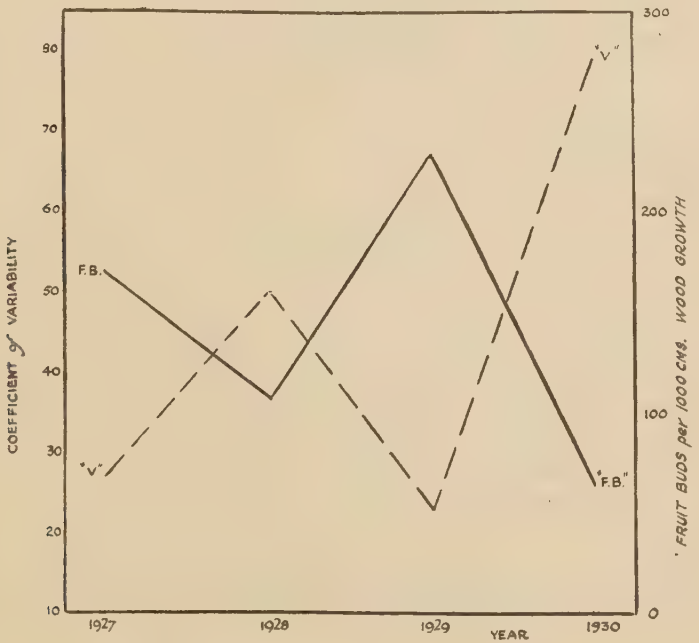
(*b*) *Other outside influences.*

Once planted many factors may influence the behaviour of trees, some of which are unavoidable, while others may be avoided by constant observation and the greatest care in the management of the plantation.

Such necessary operations as pruning, thinning, and spraying probably tend to make the trees more even, though they may lessen the differences between treatments, since it is seldom possible to treat each differently treated tree on its merits and a standard method is usually adopted for all of the same variety.

FIG. III.

The effect of Biennial Bearing on Crop Variation, as shewn by the fruit bud formation of Lane's Prince Albert apple on a known rootstock in four consecutive years.



The occurrence of disease or other damage may as a rule be prevented by careful management, but it is bound to occur sometimes and it is of the first importance that such happenings should be carefully noted, since they may be the primary cause of that bane of pomologists, the tendency of apple trees to bear heavy crops in one year and little or nothing the next. This may not seem to affect the variation very greatly at first sight, but unfortunately apparently

identical trees are apt to start the habit in different years. Thus, while the variation over a period may be unaffected, that in individual years is greatly increased. Fig. (iii) shows the effect of this habit on the variation. Whereas in a year of generally heavy fruit bud formation the variation in cropping is low, the presence of one or two trees which are out of step with the others makes that in the " off " year very high indeed.

Improvements in Material.

It is quite clear that drastic measures were needed if horticultural field experiments were to be of any practical utility. The first improvement which has been effected has been in material. The classification work which was carried out on the small fruits of this country has already been referred to (55) (50).

With regard to the tree fruits, some workers have endeavoured to remove the potential source of variation in the seedling rootstock altogether by propagating the trees by vegetative means on their own roots. The methods in use have been described recently by Hatton (61). While in common use for certain plums in this country, the raising of commercial apples in this way is at present of doubtful practicability, if it is indeed desirable.

The standardization of the rootstocks seemed therefore the most practical way in which to obtain a greater uniformity, and, in the paper quoted above, Hatton has given an exhaustive account of the way in which this has been done. One or two examples have already been given, which show that trees on vegetatively raised stocks are more uniform than those on seedlings.

Although few direct comparisons are possible the experience at East Malling with standardized stocks leads to the belief that by this means the variation has been greatly reduced ; thus in the first year of full cropping four sets of trees of Lane's Prince Albert at 10 years of age, on widely different rootstocks, had coefficients of variability of 23%, 26%, 16% and 29%. These all compare very favourably with Batchelor and Reed's 41% for the cropping of Jonathan apples on unknown rootstocks at the same age.

These same four sets of trees serve to drive home the case in favour of the abolition of the seedling rootstock, since each of the rootstocks here in use is a clone derived from a seedling in the original collection made at East Malling, and while in eleven years trees on one of them (No. XVI) have averaged over a thousand metres of wood, those on another (No. IX) have only averaged 170 metres.

The cropping history has been equally different, for in the first 5 years those trees on the vigorous rootstock had borne only half a pound of fruit per tree, while those on the dwarfing stock had already averaged 18 lbs.

That these differences are not merely quantitative has been emphasized by Hatton (61), each stock communicating a set of characteristics, such as habit of growth, anchorage, or resistance to disease, which are indelibly stamped on every tree built up by this particular combination of stock and scion.

If further evidence need be given, the reader may be referred to the results of a manurial trial of apples recently published, wherein with units of 8 or 16 trees it has been possible to prove not only that in 10 years a balanced manurial treatment has proved superior to starvation, but also such finer results as the different response to manuring of trees on one rootstock compared with those on another (1). This goes far to explain the contradictory results obtained in fertilizer trials where the rootstock was *unknown*.

II. THE LAYOUT OF HORTICULTURAL EXPERIMENTS IN THE FIELD.

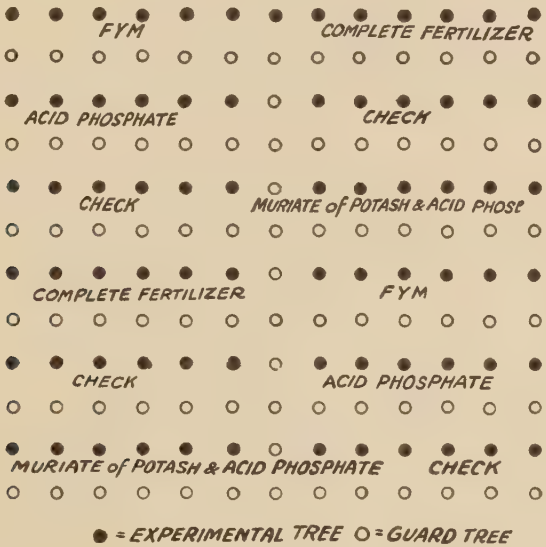
As the difficulties of horticultural field experiments became more obvious, the tremendous variation in material, and the effects of soil heterogeneity, certain refinements of Pickering's method of layout were evolved, most of which were designed to allow for or reduce the effect of soil variation ; thus Lyon, Heinicke and Wilson describe a short experiment on young trees, in which the plots consisted of 20 trees, each similarly treated plot being repeated four times (78).

Again Hedrick and Anthony have described a long period fertilizer trial of apples, in which the plots were duplicated (62), and as this is typical of many trials of the period, a plan of the layout is given here (Fig. iv.).

The earlier stock trials at East Malling were planned in somewhat the same way, a unit of 20 trees being aimed at, these trees being distributed as far as possible in at least four situations on the plot (58).

Other workers have planned the distribution of their treatments on the orchard in accordance with observations made on their trees for a long period prior to the experiments (10). This method is still strongly advocated for use with tropical crops, such as cacao and coconuts, the trees of which exhibit extreme variability. Thus, at the Toklai Experiment Station, Assam, the yields of tea plots were recorded for 2 years before selecting for each treatment blocks of 5 to 12 plots having the same average yield (28). In Trinidad the annual yields of a great many cacao trees on the River Estate have been recorded for a period of at least 10 years and these trees only are used for experimental treatment.

FIG, IV.



In 1908 Student drew attention to the danger of using mathematical tables designed for use where the number of observations was very large for testing the reliability of the results of experiments where the numbers were small, and produced tables for use with such small numbers (106).

Most of the methods used by the next generation of workers are based on a method, of which this writer was the originator, and which is known accordingly as " Student's method " (107). In its simplest form, the field is divided into a number of pairs of plots, and in each pair of plots the two varieties under trial are grown side by side. The differences between adjacent plots are then considered and thus the effect of soil variation minimized.

In agricultural trials the method has been very widely used, plots being laid out systematically, e.g., ABABAB, or later, using Beaven's half drill strip method, in sandwiches ABBA, ABBA, ABBA (11).

Sax (100) strongly recommends the use of this method in horticultural experiments, and Anthony (4 and 5) outlines the scheme of a fertilizer experiment designed in such a way that Student's method can be used to analyse the results.

These arrangements are, of course, limited to the testing of two varieties ; but Engledow and Yule (37) have described a further modification for use in chessboard trials involving a number of varieties, based on Student's method. In this case the plots are arranged :—

ABCDEAB
CDEABCD
EABCDEA
BCDE etc.

This method, which was designed for use with very small scale cereal trials, has not been used very frequently in horticulture, though Kortleve (76) has designed his tea manurial experiments in this way.

Another method of layout, which has been extensively used abroad, is known as Faulkner's method (39). In this case every other plot is a control plot ABACABACABAC and treated plots are compared only with the two adjacent controls, the error being calculated from the control plots. While this has proved valuable in certain cases in tropical countries, it is somewhat limited in its applicability and involves a large proportion of control plots, though this may not be a disadvantage in countries where land and labour are cheap. The particular advantages of the method for West African conditions, when carrying out field trials with annual crops, have been described by Faulkner recently, but no claim is made that it is the best method for complicated manurial trials or for permanent crops (40).

Randomization of plots.

All these methods of layout are of a systematic nature. During recent years, however, it has been shown by R. A. Fisher that such systematic arrangements, and in particular those in which care is taken to put unlike plots as close together as possible, do not afford a valid estimate of error. He lays down that, " The estimate of error " (by which differences between unlike plots are judged) " afforded by the replicated trial depends upon differences between plots treated alike. An estimate of error so derived will only be valid for its purpose if . . . pairs of plots treated alike are not nearer together or further apart than, or in any relevant way, distinguishable from pairs of plots treated differently." The best way of making sure of a valid estimate of error is to arrange the plots deliberately at random (41).

Student heartily endorses this opinion : " The experiments must be capable of being considered to be a *random* sample of the population to which the conclusions are to be applied." (105).

Student's original method involving two varieties or treatments can be made to conform with this principle very easily. In the first case, in selecting the pairs of varieties, the position of each within the pair is decided by the spin of a coin, e.g., AB, BA, BA, AB, AB, AB. In the second, each sandwich is considered as a unit, the two possibilities being ABBA and BAAB. These may then be randomized in the same way, e.g., ABBA, ABBA, BAAB, ABBA, a course which has much to commend it since it is quite possible by purely random selection to get a much greater proportion of the A's on the left of B than on the right, thus giving an undue advantage to one variety, should there be a fertility trend from left to right.

It must be remembered, however, that with permanent crops it is seldom that trials involving only two contrasting treatments are required. Student's method is therefore limited to such experiments as spraying or cultivation trials, where there are only two treatments and one variety in question. It may also be employed for extension trials, in particular of vegetables, where two varieties are being tried out in a large number of different centres (68).

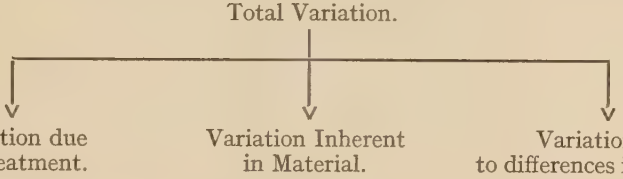
Where three or more treatments or varieties are in question, one way of arranging the trial would be to scatter the trees or plots deliberately at random in the field. While this is perfectly legitimate, the estimate of error calculated from trees treated alike would include inseparably, not only the variation inherent in the material, but also that due to differences in position.

Now Fisher's methods are designed in such a way that these two component parts of the total amount of variation, or variance, between plots treated alike can be estimated separately, while the element of chance is maintained. Thus in the method of randomized blocks the field might be divided into four blocks, each containing one plot which receives each treatment, but

within the block the position of each treated plot would be decided by chance. Thus supposing four varieties were concerned, each to be repeated four times, they might be arranged : BCDA, CABD, DBAC, DABC. The variance within each of the four sets of plots treated alike can then be calculated. This still includes both the sources of variation mentioned above, but in this case if the variance between the yields of the four blocks is calculated, an estimate is obtained of the amount of variation contributed by positional differences. This can be separated from that obtained above, leaving only the variation inherent in the material.

There is, however, a further source of variation in the whole experiment, namely, the variation between the yields of plots treated differently, or in other words the increase in the amount of variation in the whole experiment caused by the treatments. This will be great or small according as there are big or small differences in the results of the treatments, and can be calculated in exactly the same way as the variance due to differences in position from the mean yields of the four sets of differently treated plots.

To sum up, in an experiment there is a total amount of variation which is made up of three parts :—



The method of treating the results of the experiment is known as the " Analysis of Variance," and all Fisher's methods of plot arrangement are devised so that the data may be analysed in this way.

Tables have been prepared so that the differences between the variance due to the different component parts can be tested (42). Thus if the treatment variance is significantly greater than that inherent in the material, the treatments can be said to have given significant results and the magnitude of the difference can be easily discovered from the mean yields.

Fisher's basic methods of plot arrangement are known as the " Randomized Block " and the " Latin Square " methods. The simplest form of the first method has already been described ; the arrangement is, however, very adaptable and the plots may be any shape or size, and the blocks arranged in any way, so long as each block is as compact as possible and contains one plot devoted to each treatment, the actual position of which within the block is determined by chance. Thus, if something of the soil variation is known, the blocks can be arranged so as to sample that variation to the best advantage. Examples of adaptations of the method to horticultural material are given in a further section of this paper.

The latin square introduces a slight further restriction on the completely random arrangement ; in this case there are as many replications as there are treatments, and the plots are arranged in the form of a square, in such a way that each differently treated plot occurs once in each row and once in each column. For example with five treatments there would be five replications, and they might be arranged thus :—

E A B D C
A D C B E
C B D E A
D E A C B
B C E A D

The element of chance is maintained by selecting at random one such arrangement *from all the possible latin squares* containing the same number of treatments, each time an arrangement is required. Thus for a 4 x 4 Latin Square there are 576 possibilities, and for a 5 x 5, 161,280.

One further modification of the randomized block method may be described. This method seems of particular value to horticulturists in that it insures the better distribution of the plots on the ground, without necessitating the large number of plots in a latin square with eight or more treatments. The plots are arranged in randomized blocks with the restriction that each plot must occur not only in each block but also in each column. This is shown in fig. (v).

FIG. V.

Four randomized blocks of eight treatment arranged so that each treatment occurs once in each column as well as in each block.

A.	C.	D.	H.
E.	B.	F.	G.
C.	F.	H.	E.
G.	A.	B.	D.
D.	E.	G.	B.
F.	H.	C.	A.
B.	G.	A.	F.
H.	D.	E.	C.

It is not proposed to go further into the details of the theory on which these methods are based here, but a few examples of the use of the randomized block method in horticulture are given at the end of this paper. Readers wishing to study the methods in more detail are referred to accounts given by Fisher in his book (42) and elsewhere (41), and to the detailed account, in which are included examples of both methods, given by Fisher and Wishart in a memorandum issued by the Imperial Bureau of Soil Science (43).

Other writers have described these methods in relation to different problems (67, 68), thus Student gave a short account to American readers (105), Maskell has published a detailed paper on the subject in Trinidad (80), and Horton Beckett has used them in the Gold Coast (12, 13).

The advantages and disadvantages of Fisher's methods, especially the randomization of plots, have provoked much discussion in recent years. In particular, during the last year sessions have been devoted to the subject of the arrangement of field experiments at a number of different conferences; notably at the Imperial Horticultural Conference in London, August, 1930, (21, 69, 79), the Conference of West African Agricultural Officers in the Gold Coast, October, 1929, (13 and 40), the Conference on Cotton Growing Problems at Didsbury, August, 1930 (81), and the Conference on Soil Science Problems at Rothamsted in September, 1930.

Although most experimenters who have tried randomized methods of plot arrangement have found that the advantages very quickly outweigh the disadvantages, there are still some who think that randomization is not a practical proposition, especially in countries where native labour of a low order of intelligence has to be relied on.

Thus, apart from those who decry the method of field experimentation altogether and who would rely on the field survey for the improvement of agricultural and horticultural methods (34), Faulkner and Mackie, after examining the various modern methods of plot arrangement, conclude that in West Africa the methods generally used on English experimental farms are unsuitable, except for very special purposes and in circumstances which are exceptional (40). Again, Iliffe and Viswa Nath come to the conservative conclusion that having reviewed "the literature advocating randomization in selection of plots under trial," they consider that "much more must be said on this subject before we can confidently recommend the adoption of randomization for general use" (72).

Since these writers are referring in the main to agricultural conditions, and since some of the criticisms which they make apply with even greater force to permanent crops, it seems worth while enumerating some of these disadvantages of the method and suggesting means by which they may be overcome.

Disadvantages of randomization.

(*i*) Errors of the " damn fool " order are more likely to occur in randomized experiments. This form of error is liable to occur in all horticultural experiments, and it seems to the writer that its avoidance is simply a matter of good organization and careful demarkation of plots. At East Malling several large scale trials, in which Fisher's methods of layout have been used, have been in progress for three years and ordinary unskilled labour has been used for picking, spreading manure, cultivations, etc. Up to the present such errors as have been made might equally well have occurred with systematic arrangements, such as picking guard rows in with the plots, the loss of a box of apples before recording, or misreading a scale.

It is true that in some tropical countries native labour of an exceedingly low order of intelligence has to be employed and that communication between the experimenter and the natives, who may speak two or three different languages and some of whom cannot even count, is very difficult. In such cases any experiments must necessarily be of the simplest possible character and special devices must be thought out for such operations as harvesting. In this connection the writer is indebted to Dr. W. S. Martin for an ingenious device for harvesting his coffee experiments (laid out in latin squares) in Uganda, where none but native labour is employed. The trees in each differently treated plot are marked with a different coloured band and when picking, say, the red plots, the pickers are given a red armlet ; it is thus easy to see at a glance if a picker is in the wrong plot.

Where individual trees are being picked, errors may be avoided if the number and weight is recorded under the tree, as soon as the crop is picked.

(*ii*) Randomized experiments make bad demonstration plots.

The necessity of combining the two functions of trial and demonstration, has been the ruin of many otherwise admirable experiments.

Accurate and economically planned experiments do not and never can make very good demonstrations (68). On the other hand, it is conceded that the results of experiments are best explained to the farmer by concrete examples. Indeed, in tropical countries, where it is not possible to lay out special demonstration plots, the demonstrative value of an experiment may be all important. For, not only must the farmer and the " visiting agent", who often controls his actions, be convinced—and undoubtedly this is best done by ocular evidence—but also the very life of the research station may depend upon the impression made upon the local government, the non-official members of the legislature and the general public, as all these hold the purse strings. There is no doubt that special demonstration plots involving comparatively large numbers of trees achieve this best. If, however, the two must be combined, it seems to the writer that just as good results can be got from randomized plots as from systematic. Thus randomization makes no difference to the demonstrative value of Student's method for two varieties and randomized blocks placed end to end are just as good a demonstration as Faulkner's method, with the additional advantage that all comparisons can generally be seen side by side, whereas with Faulkner's method each treatment is next to the control only.

(*iii*) Phenomena, which are obvious where there are blocks of trees treated alike, pass unnoticed if individual trees are scattered at random.

This is the most serious disadvantage of randomization of tree crops and it can only be overcome by increased care in observation. Thus, such occurrences as a difference in time of blossoming or leaf fall must be deliberately looked for and not relied on to show themselves.

(*iv*) The management of trees or plots scattered at random is more difficult.

In planning a horticultural trial this factor must always be borne in mind ; it is obviously no use submitting a plan that is impossible to carry out in practice. In order to fit in with practice, therefore, modifications in design can often be made. Thus in a rootstock trial involving four rootstocks and four varieties of plum, the complete randomization of the 16 combinations of variety and stock would have made such operations as picking and spraying very difficult.

In this trial, therefore, a modification of the randomized block method was introduced, and in each row two varieties only were planted alternately, each pair being selected by chance, and the rootstocks completely randomized; fig. (vi) shows a section of this plan. Where land is cheap, this trial would be further improved by the insertion of guard trees to ensure a similarity of root competition throughout the trial.

FIG. VI.

Plum Variety and Rootstock Trial at East Malling.

Portion of plan showing arrangement of trees in randomized blocks—with the modification that two varieties only occur alternately in each Row.

V. / O.	C.G. / C.P.	P. / C.M.	B.V. / M.
P. / O.	B.V. / C.M.	C.G. / O.	V. / C.M.
V. / C.P.	C.G. / M.	P. / C.P.	B.V. / C.P.
P. / M.	B.V. / O.	C.G. / C.M.	V. / M.
V. / C.M.	C.G. / M.	P. / M.	B.V. / C.M.
P. / C.P.	B.V. / M.	C.G. / O.	V. / C.P.
V. / M.	C.G. / C.P.	P. / O.	B.V. / O.
P. / C.M.	B.V. / C.P.	C.G. / C.M.	V. / O.

Varieties
V. = Victoria.
B.V. = Bastard Victoria.
C.G. = Cambridge Gage.
P. = Pershore Egg.

Rootstocks
O. = On own Roots.
M. = Myrobolan.
C.P. = Common Plum.
C.M. = Common Mussel.

In another case, a cherry variety and stock trial, the trees had to be planted so that every tree had an equal chance of pollination. This was overcome in much the same way as the above.

In soft fruit trials the shape and size of plots may be adjusted to conform with easy cultivation without upsetting the chance distribution.

(*v*) The mathematics are too difficult.

This criticism is hardly a serious one, since a lack of basic knowledge of the mathematical theories involved in no way prevents the investigator from analysing his results correctly by the light of elementary algebra and arithmetic.

Fisher and Wishart have already shown the working out of several agricultural trials (43), and in another section of this paper the results of certain horticultural experiments are worked out in detail.

Advantages of randomization.

The advantages of Fisher's methods are on the other hand very great. They offer :—

(i) A valid estimate of error.

(ii) The elimination of the soil variation in the analysis by the perfectly legitimate means of estimating its extent.

This together with standardized material makes possible an accuracy previously undreamt of. Already it is quite possible to prove 10% differences with small fruits, and there is no reason why a similar accuracy should not be achieved with trees.

(iii) A greatly enlarged scope for field experimentation. In the older type of field trial it seemed necessary to keep the issue as simple as possible ; nowadays, with this new weapon, it is possible to combine in one experiment a number of different issues. Thus Fisher suggests : " Nature will best respond to a logical and carefully thought out questionnaire ; indeed, if we ask her a simple question, she will often refuse to answer until some other topic has been discussed." (41).

For example, in an apple manurial trial, the answer may be a straightforward one, e.g., potash or nitrogen ; but it is much more likely to be a combination of nitrogen and potash. Further, the answer may be different if the rootstock is varied, and yet again the correct result may only be achieved, if the nitrogen and potash are put on at the right time of year : here is an experiment involving four factors, rootstock, potash, nitrogen and time of application, and it is quite possible or even probable that truth may only lie in the interaction of all these factors.

The analysis of variance can give an answer to all these questions, for in exactly the same way as the total variance is split up into its component parts, treatment, position and inherent variation, so the treatment variance can be split up : e.g., if two varieties A and B are being compared on two stocks I and II, there are four different combinations of variety and stock, A.I, A.II, B.I, and B.II ; and the variance between these four is split up into that part due to a difference between A and B, that due to the difference between I and II and that part which is caused by the interaction of A and B with I and II. And this interaction, if, as so often happens in horticulture, A.I is greater than A.II, but B.II is greater than B.I, is the most important part of the experiment.

Maskell emphasizes the enormous importance of these multiple factor experiments with cotton—an excellent example of which was carried out in the Sudan recently (47)—and shows how one comprehensive scheme can be prepared involving not only different treatments of the crop, but differences in place or season (80).

This experiment, which involved the whole time of two technical officers, could, of course, only have been done under conditions, such as obtained in the Sudan, where the water supply was under perfect control. Nevertheless the value of such experiments which can answer a large number of questions at one time, especially with permanent crops, cannot be underestimated.

III. FIELD EXPERIMENTS WITH PERMANENT CROPS IN TROPICAL AND SUB-TROPICAL COUNTRIES.

Whatever opinions they may hold as to the merits of Fisher's methods for application in the tropics, research workers there are admittedly confronted with difficulties peculiar to their situation and to the crops with which they have to deal. These difficulties will now be briefly considered.

The more important tropical and sub-tropical crops upon which experimental work is being done can be readily divided up into two broad groups (21).

(i) Those which are normally self-fertilized and which are presumably homozygous.

(ii) Those which are cross-fertilized and heterozygous.

The second group may be further sub-divided :—

(*a*) Those crops which are capable of being reproduced by vegetative means.

(*b*) Those which from their nature are necessarily reproduced from seed.

(i) The first group is unfortunately the smallest, though some experimenters include in it one crop of primary importance, namely Arabian Coffee. If it is rightly grouped here, it

would offer no special difficulties to the research worker, except with regard to shading experiments. But though trees of this crop are reputed to be very even, there would seem to be few data available by which their actual variability can be compared with that of other crops. Further, the amount of cross pollination to which the trees are subject still seems a matter of opinion.

(ii) *a*. In this sub-group are found such important crops as cacao, citrus fruits, rubber and cinchona, which can all be propagated by budding or grafting, and tea, dates, bananas, pineapples, and tapioca, which are usually propagated from cuttings, slips or suckers. Rubber, cinchona, tea and tapioca are included as being subject to much the same rules of experimentation as trees cultivated for their fruit.

In this class, by far the most important problems at present are those which concern the standardization of the material with which experiments are to be carried out. Cheesman says of ordinary plantation cacao, that at the present time it is a seedling crop exhibiting a range of variability probably at least as wide as that covering all the existing horticultural varieties of apples (21).

These remarks apply equally to the vast majority of crops in this class, as grown to-day. Citrus fruits, indeed, have advanced one step further and definite varieties are known and propagated by budding on to seedling rootstocks ; but as long as this unknown factor of the seedling stock is allowed to remain, with the possible exception of the West Indian lime, which is said to breed true from seed, even this large class of fruit trees will not be a really satisfactory crop for field experiment.

An interesting possibility in connection with citrus fruits is the utilization of the phenomenon of polyembryony—a common occurrence in this class of fruit trees. By this means it may be possible to raise vegetative families from seed, as long as the sexual embryos can be recognized soon after germination and removed. These possibilities are already being explored (21).

The process of standardization has also already begun with cacao and rubber, and clonal varieties are being established (21, 45, 66) and propagated by budding. That the standardization of the scion varieties is only the first step on a long journey, has, however, already been shown by Harland, who, referring to cacao, says : " There is no method of telling whether a tree will transmit heavy yield either to its budded or seedling offspring except by testing it ". The next step is once more the elimination of the seedling stock, and research work having this end in view has already been started (54).

(ii) *b*. In this sub-group there are two tree crops of major importance—coconuts and oil palms. Again a tremendous variation exists in the trees. Jack, who has given a considerable amount of information on the subject of coconuts (73, 74), shows the variation coefficient of this crop to be usually about 37%. With regard to oil palms, a good example has been given by Rutgers, who has published the individual tree yields of some 135 palms over a four year period, and these show variation coefficients of from 45% to 59% (95).

The standardization of such material must necessarily be a much longer and more uncertain process than where vegetative propagation is possible. Already breeding work and selection of seed from heavy bearing parents have been started, but it will be a long time before really uniform trees are a practical possibility.

In addition to variable material, tropical workers have to face many difficulties which are not usually met with in temperate countries. These include aspect, gradient, the necessity for shade trees, artificial means which have to be adopted to check erosion, such as silt pits, contour drains, terraces and contour hedge crops. Again the necessity for following the contours on steep lands to prevent erosion increases the difficulties of spacing and plot arrangement.

Experimentation with variable material.

In the meantime, until these various processes of standardization are accomplished, and in the face of these difficulties, are field experiments with such variable material worth while ?

The answer to this question is most definitely "yes", provided the experimenter is prepared to take enormous trouble to get his results and refuses to be hurried. But if such conditions

cannot be fulfilled, then, in the writer's opinion, the experimenter would be better advised to postpone the elucidation of such questions as pruning, manuring and cultivation, and concentrate on the task of standardization.

The importance of individual tree records with deciduous crops has already been stressed. With such variable tropical crops as are now under consideration it is the only possible way to get reliable results. In such experiments every tree is a separate entity, and such a quality as a heavy or light bearing capacity is only one of the many ways in which it may differ from its neighbour. The only way, therefore, in which a general answer can be obtained to such a question as the effect of a particular fertilizer upon cacao is by observing the effect of that manure upon a large number of individuals, and comparing their performance with that of an equally large number of individuals from which that fertilizer has been withheld.

This information can be obtained with greater accuracy and with fewer trees in some cases, if the performance of the trees is known for some years before the differential treatments are begun. On the River Estate, Trinidad, large numbers of cacao trees have been recorded for a ten-year period, and it has been definitely shown that heavy bearing trees as a rule remain heavy bearers and light bearing trees remain poor croppers (45).

This fact is said to be true for several other crops including coconuts and oil palms, but a word of warning would seem to be necessary here.

Such a record, if it is to be of any value, must be kept for a sufficient number of years, and the bearing capacity of young trees should not be taken as a criterion of their future performance, e.g., coconuts cannot be considered mature until five years after the plantation has come into full bearing.

Basing his deductions on the yields of cacao recorded on the River Estate, Freeman considers two years record the minimum period, provided the cropping is undisturbed, but other workers have, in conversation with the writer, suggested a considerably longer period than this.

With regard to coconuts, it must be remembered that most plantations are uncultivated, and the mere fact of bringing a plantation into cultivation for the purpose of a manurial trial will have a considerable effect on the trees. This crop then should be cultivated and recorded for at least five years before the manures are applied. With apple trees it is a characteristic of certain combinations of scion and rootstock that they start to bear heavily early in life, while others do not really reach their full bearing capacity until more than 10 years old. There seems every possibility that the same phenomenon may be apparent in the heterogeneous collections of seedlings or clonal varieties on seedling stocks at present under consideration. Thus in the case of the oil palms previously referred to (95), though the correlation between successive years' yield in the 5th, 6th and 7th years was always positive, it was not until the eighth season that the yield was very highly correlated with that of the previous year.

The problem of arranging such experiments is rather complicated. If the variation is not very high except in yield, ordinary randomized blocks may be used, and the data analyzed, not on the basis of actual yield, but taking as the variable the rate of increase over the previous record of the tree. Lord and Abeyesundera have used what they term the " Percentage Increase " method for plots of rubber trees (calculating the percentage increase of each plot over the previous year's yield), and have obtained thereby seemingly very accurate results (77).

They recommend the use of 24 tree plots replicated in randomized blocks, laying down quite rightly that this percentage increase method is of no use without replication. The writer, however, will go further in saying that with seedling trees, even though, as in this case, they may have one parent in common, the method may prove definitely misleading unless the variation of individuals is taken into account.

The high variation in young rubber trees at the Rubber Research Institute of Malaya shown by Haines (51, 52) further enforces this argument.

This worker has laid out a large scale manuring experiment on rubber trees using the latin square method of arrangement and plots of 81 trees surrounded by a guard row.

In a recent paper he suggests that " plots should be adjusted to the natural unit for estate recording ", but for trials other than manurial trials individual tree records are recommended (53).

The use of randomized blocks for individual tree plots is apt to entail difficulties, as will be shown in the third example in this paper, for though such a method may be suitable for such management experiments as pruning, for manurial, shading, spacing or cultivation trials larger plots are necessary.

It seems, therefore, that the only possible way to get satisfactory results is to use comparatively large plots such as these workers suggest, and to consider not only the variation between plots, but also that within plots by taking individual tree records.

Another difficulty which may make the ordinary use of randomized blocks or the latin square of little value is the nature of the soil heterogeneity. Martin and Beckett in a recent paper say of West African conditions : " In our experiments we are not concerned with major soil differences such as those due to different soil formations, but to minor differences due to causes such as previous cultivations, positions of ant hills, dung heaps and minor differences of level and drainage " (79).

In Ceylon again, experimental work has to be done on ground of all levels which has been cultivated in very small holdings for generations.

For such cases, Martin and Beckett have suggested a modification of the randomized block method, when treating trees of known history, by which the blocks are allotted, not to different positions of the ground, but to differences in yield capacity. Thus, the trees are grouped in categories according to their previous yields, and a block containing each treatment at random is allotted to each category (79). This method involves, of course, individual tree plots.

The necessity for guard rows is mentioned in other parts of this paper. With tropical crops these are no less essential, and if possible some tree of uniform size and root range should be used. This applies especially to experiments involving such crops as coconuts and rubber where the root range is very great. Sampson has shown that where manure was applied to a strip of 8 ft. down the centre of the middle of five rows of coconut palms, while the row which received the manure gave the largest increase in yield, both those immediately adjoining it gave a very appreciable increase also (96).

The possibility of utilizing shade trees in the guard rows, especially where shade is not the immediate object of the trial, is worth consideration.

Finally it must be repeated that a field experiment with a heterogeneous crop is a complicated and precarious business calling for the utmost care and patience. There are, however, definite signs that standardization of material, with many of the major tropical and sub-tropical crops, will, in time, become an accomplished fact. The growing appreciation that it is the most vital necessity for the improvement of these crops gives definite hope for the future.

IV. TECHNIQUE OF FIELD EXPERIMENTS WITH PARTICULAR REFERENCE TO DECIDUOUS TREE FRUITS.

Minute accurate observation of the behaviour of each individual tree in his experiment is no less important to the horticultural worker than accurate trap-nesting to the expert poultry breeder or milk yields to the up-to-date dairy farmer. Hodgson (70) in fact recommends even the ordinary grower of citrus fruits to keep simple crop records for individual trees in order to increase the efficiency of his orchard, and Clark Powell (23) advises South African growers to adopt the same method.

If a knowledge of the individuality of his trees is important to the grower, to the research worker it is an absolute necessity. With such mixed material as some tropical fruits or other trees on mixed rootstocks, this may be the most important factor of all (69), and even though the material may be uniform at the start, each tree is a separate entity, which must stay where it is planted for many years and cannot be easily replaced, should some accident occur to make its behaviour differ from that of its fellows.

The research worker, however, cannot confine his observations to simple crop records. For, if he is to understand the cropping performance of his trees, he must have a sound knowledge of their vigour, since, even though the most vigorous tree does not necessarily produce the heaviest crop, the two characteristics are always closely connected.

The numerical records which he must take can be conveniently classified under the two headings, vigour and productivity, and since the methods by which the information is obtained are often highly technical, it seems worth while to describe in some detail the tree characters, which various workers have measured and the difficulties which have been met.

1. Vigour Measurements.

(*a*) *Small Fruits.*

With small fruit crops such as strawberries, raspberries and currants, few attempts have been made to record the vigour of the plants until recently. At East Malling, however, Rogers (93) has recorded the average spread of strawberry plants by means of a plant to plant survey. For this purpose a T-shaped rod has been used, the cross piece being graduated in twentieths of a metre (Fig. vii). In this, as in all measurements, the recommendations of the biological measurements committee of the British Association have been observed; that is to say, " by using length classes bounded by the divisions marked on the measuring instrument used, instead of the more common practice of using length classes centred on visible divisions, and bounded by imaginary ones " (15).

Thus the spread of the plant would be recorded as being between 6 and 7 twentieths of a metre, rather than 6 or 7 twentieths which ever is the nearer. Actually, for the sake of brevity, if the reading were between 6 and 7 twentieths it would be written down as "6", the nearest graduation below, the average for the whole series of observations being adjusted afterwards.

The vigour of raspberries in a manurial trial has been estimated by measurements of cane length and counts of the number of canes per plot. From this the average length per cane has been taken to represent the vigour.

Though laborious, such measures as these are simple to take and it seems likely that their use will become more frequent, especially in manurial trials, where it is possible that the applications may effect the vigour of the plant, while showing no influence on crop.

(*b*) *Tree Fruits.*

Pickering, in his experiments at Woburn, recorded the mean height and spread, girth of stem, weight of prunings, length of new shoots, stoutness of shoot (i.e., weight per unit length), weight of total leaf crop, weight or size of leaves obtained from measurements of selected leaves, and the nitrogen assimilated by the leaves of his trees (85). He says : " These various measurements were of course not always equally applicable or convenient, nor could they always be expected to give values even in the same direction. . . . But it has been well established by the observations themselves that all these measurements yield results of the same character in ordinary cases, and that any one of them may generally be taken as a criterion of the condition of the tree " (85).

In America, the girth of stem has been the most widely adopted measure of vigour ; indeed Anthony and Sudds declare that, " trunk and branch circumference are so closely correlated to weight and elongation, that usually they are the only records which need be taken in studies of apple tree vigour " (6). There has, however, been much discussion on the subject and other writers express more doubt as to the efficiency of girth measurements alone.

Thus Chandler gives a table showing the adverse effect on girth increase of a heavy crop (18) Heinicke says that " It appears that, though the coefficient of correlation between girth and size is high, the relationship is of such a nature that circumference measurements are reliable as a measure of small differences only when rather large numbers of approximately similar trees are concerned in the average " (65). Again Roberts notes that " it is not safe to assume that quantity and quality of lateral growth are always correlated" (88); and Waring concludes that, though there is a high correlation between total crop and girth of stem, this correlation is affected by different conditions and is lessened with age (112).

In a valuable paper recently published, Collison and Harlan have shown the trunk circumference of 21 McIntosh apple trees to be closely correlated with the total weight of the above-ground portion of the tree, and conclude that " mean trunk circumference is therefore a reliable index of tree size, at least among the trees studied." (27).

Blake and Hervey (16) emphasize the value of twig growth in peach trees and, noting that the girth is not a satisfactory substitute, have elaborated a method of obtaining an approximate value for this character from the total number of shoots and the number over 24 inches in length.

Sax and Gowen, in a study of 881 twenty-eight year old Ben Davis apple trees, stress the value of " tree type " as an indication of productivity (97, 98), but, " by determining the correlation between type and yield, with the circumference held constant, little or no correlation between type and yield is found. From this it may be concluded that yield is, in general, dependent on the size of the tree and that differences in size of trees of the same age are closely associated with a rather definite type of growth " (97).

At East Malling nearly all the measurements taken by Pickering have been made at one time or another (58). In particular, the annual twig growth, girth of stem, and height and spread have been recorded on large numbers of trees over a period of at least eleven years. The following remarks summarize the experiences there.

(1) No single measure is ever sufficient to describe the vigour of a particular tree.

(2) Whilst most vigour measurements, which are the sum of a long period of growth, e.g., girth of stem, total twig growth, height and spread, are positively correlated, the actual relationship between these characteristics may be altered by differences in build up, treatment, or conditions.

(3) The area of the cross section of the stem is more closely correlated with the wood growth than the girth itself.

(4) In any one year the increase in cross section is so much influenced by the crop, that it gives little indication of the increase in the other characteristics of vigour (69).

Thus the experimenter must be ready to take any or all of these measurements ; the following notes describe the difficulties which have been met at East Malling and some of the methods which have been used to overcome them.

(*a*) *Annual Wood Growth.*

The annual increment of new wood is probably one of the most valuable manifestations of tree vigour. It is, however, the most laborious to estimate, entailing the annual measurement of up to a thousand twigs per tree in some cases, and the subsequent addition of these entries to find the total tree growth for the year. With young trees this is not a very difficult affair, but with older trees, only by the adoption of labour-saving devices and a systematic method of measurement can it be done at all.

At East Malling the work is carried out by two recorders. One of them, who does the actual measurement in the field (Fig. viii), is connected to the laboratory by means of a field telephone ; the other recorder, who remains indoors, takes down the measurements on a Burroughs adding machine, which automatically types each entry and presents the total when the tree is completed.

In measuring the tree, the number of main scaffold branches is first counted and the topmost branch selected for the start. The new shoots are then measured, using a metre rod graduated in centimetres, starting at the top of this branch and working systematically downwards until the whole branch is done ; the end of each branch is always noted. When complete, the measurement is checked by recounting the main branches and checking this against the entries in the record.

Though with young trees this record will not take longer than five to ten minutes a tree, with 10 year old trees it may take up to an hour.

Such an operation can only be undertaken with comparatively few trees after they are six or seven years old, and then only on trees spur pruned annually ; if an estimate of twig growth is still necessary therefore, certain substitute records have to be relied on. Blake's method for peaches has already been alluded to, and with unpruned trees some such procedure must be adhered to. With spur pruned trees, such as are common in this country, a very fair estimate can be arrived at from the prunings. The number of prunings and their weight are in themselves of value as a measure of vigour.

FIG. VII.
Measuring the spread of Strawberry plants.

FIG. VIII.
Measuring wood growth in the field on a seven year old apple tree.

FIG. IX.
Pruning secateurs with automatic counter.

2. Cropping.

(*a*) *Fruit Bud Formation.*

The first measure of productivity of interest to the research worker is the amount of blossom produced by the tree. Roberts (88) stresses the value of this character as a very necessary adjunct to crop.

At East Malling, the number of blossom trusses has always been recorded, where possible, in all experiments with tree fruits (58, 59, 60), both as regards time of opening and as a measure of productivity when weather conditions cause the failure of the crop.

In addition the proportion of blossoms which set fruit is often of importance. Thus Dorsey and Knowlton found that nitrogen applications gave an increase both in amount of blossom and the proportion which set fruit on the shorter spurs (36). At East Malling recently, on the other hand, there has been a much greater set of fruit on a plot starved of all manure for 10 years as compared with an adjacent plot receiving a balanced manurial treatment (1).

As with wood growth measurements, the difficulty of counting blossom trusses increases rapidly with the age of the tree, an added complication being the danger of injury to the buds, since often it is not possible to distinguish the fruit buds with certainty until the trees are practically in bloom, when the blossom trusses are easily knocked off. If the method of pruning and training allows, all the buds on a small number of trees can be counted, but as a rule methods of sampling have to be relied on.

A method of random sampling has been evolved recently at East Malling and employed with success upon eleven year old Bramley's Seedling apple trees which have not been tipped or spur pruned for several years.

In the winter the tree is approached from the four points of the compass and four branches selected at random.

The method of random branch selection is to approach the trees with the eyes shut until a twig is touched. The top of the branch to which that twig belongs, no matter where it is on the tree, is the beginning of the sample.

Starting from this point 500 cms. are measured off in each case and the bottom end of the sample marked with a distinctive label ; the number of the sample is marked on the label. When the blossom trusses are distinguishable, the number on each sample branch is counted. Counts are made of those fruits which set and later of those which achieve maturity.

This method, while quite practicable for bush trees, is obviously unsuited to standard trees such as are found in the U.S.A., and would have to be considerably modified. The counting of blossom trusses on standard trees must at best be a difficult undertaking.

The size of the sample branch may be varied with the age of the tree, as long as it is representative of the remainder.

(*b*) *Picking and Recording the Crop.*

Though with young trees it is sometimes advisable to make a count of fruit early in the season, the main record is taken when the crop is harvested. In addition, if an accurate record is to be kept, during the latter part of the season counts of windfalls and other dropped fruit must be made. It is not generally considered necessary to count those fruits which fall naturally and are never likely to mature, e.g., what is known as the " June drop " with apples. As a general rule " marketable size " is taken as a criterion and only those windfalls which pass this test are recorded.

Where the fruits are thinned, an approximate record of the fruits removed is necessary. Usually these are weighed for individual trees and the number estimated from the weight of a sample of from 50 to 100 apples according to the size of the thinnings and amount removed.

FIG. XII.

Portable weighing apparatus and picking bag with counter attached.

When the fruit is picked a record is taken of the actual number and weight of fruits harvested. The most important point to be noted in this connection is that a record be obtained of the number and weight of fruit *immediately it is picked* and before it is removed from under the tree. The chances of error are thereby greatly reduced.

In some cases, notably hops in this country and tea abroad, special precautions are necessary in weighing the crop to avoid errors due to differences in moisture content.

Where the crop is a large one, i.e., 3 or 4 bushels per tree or more, crop records become a big undertaking. At Malling fruit counts have been made possible by the use of what is known as the " Counting Picking Bag " (Fig. xii). Each picker is given an ordinary picking bag fitted with a counter adjusted in such a way as to record each apple as it is placed in the bag. When the bag is full, it is brought to the recorder, who weighs it on a spring balance adjusted so that the weight of the bag is allowed for (94).

The recorder is provided with a record sheet of cardboard ruled in squares, so that there is a square for each tree in the plantation and room for several entries in each square. One recorder can in this way deal easily with from 6 to 10 pickers.

With plums it is not considered necessary, nor is it indeed practicable, to count all the fruits, and sample counts are usually relied on, as with the thinnings.

Once the crop is picked it often happens that many other records are required, and if these have to be taken on individual trees, the boxes into which the fruit is picked should now be labelled carefully.

It is again an essential that as many of these records as possible be taken *in the field* ; and when sampling it should be remembered that, if practicable, it is much easier to take a random sample *directly from the tree*, before the bulk of the crop is picked, than to pick out such a sample from a box afterwards.

Grading the fruit for colour, described by Rogers (91), or such blemishes as scab on apples or pears, a method for which has been recently given by Moore (83), are records which can be made at this time.

The fruit is now taken to the packing shed and graded for size and quality.

The average size of fruit having been obtained from the record of number and weight already taken in the field, the fruit from trees treated alike is usually graded for market in bulk unless a detailed record is required. Apples are graded into six sizes on a Cutler grader, and three quality grades by eye. The standard market grades, " Extra Fancy," " Fancy " and " C " grade, give a good idea of colour and freedom from blemish, where no other record is taken.

3. Other Records.

In addition to records of growth and cropping, those all important other records, which, while less readily measureable, are none the less important, are taken as often as possible. They include such phenomena as the incidence of a particular disease or pest, suckering from the roots, and leaf scorch.

Finally there are the personal observations of the experimenter or his assistants. The records described in the foregoing have been largely of a routine nature and some of them are often taken by unskilled farm labour. It is essential that an experimenter with fruit trees should have an intimate acquaintance with his plants.

The small happenings on the plantation, this or that accident to a particular tree, the throwing out of roots from the scion, local variations in fertility or depth of soil, spray injury, etc., should all be carefully noted, for therein lie not only the inspiration for these routine records, but the means of interpreting them in after years.

V. THREE MODERN HORTICULTURAL FIELD TRIALS.

1. Black Currant Variety Trial.

This experiment is given as an example of a single factor trial, in which only one issue is in question, i.e, in this case, variety.

(a) Project.

The object of this experiment is to test the cropping of three new varieties of black currant, designated Seedlings A., B. and C. against representatives of the five groups of black currant already well known in commerce, viz., Baldwin, Boskoop Giant, Goliath, Seabrook's Black and French Black. It was particularly desired at the start that the layout of the trial should not be too complicated, and fairly large plots were desired, so that such operations as picking and spraying should be carried out as simply as possible. It was desired to plant the bushes between the rows of a raspberry variety trial and there were available sixteen rows each of which could accommodate 52 bushes.

(b) Raising the Material.

The importance of uniformity of material and freedom from disease has already been stressed in the early part of this paper, and it is always wiser in consequence for the experimenter to raise his own material. The following precautions were observed in the nursery :—

(1) Clone races of each variety were used. Thus cuttings were taken from bushes known to be descended vegetatively from a single parent bush.
(2) Cuttings were taken only from bushes known to be free from " reversion " and " big bud."
(3) In the nursery beds the young bushes were carefully rogued for " reversion " or other disease and all suspect plants removed.

(c) Planning the Layout.

(1) *Method.*

The method of randomized blocks was selected as being most suitable to the shape of the ground and the type of experiment.

(2) *Size and shape of plot and number of replications.*

With black currants single bush plots are ruled out on the score of practicability, and the question of the best number of bushes to a plot arises. This size of plot to a certain extent determines the necessary number of replications. Fortunately the results of a uniformity trial of black currants, carried out at East Malling with carefully rogued material, were available, and these were used as a basis for a decision (57). It appeared from this trial that with sixteen bush plots the coefficient of variability would not be likely to exceed 10%. Appendix B shows that nine repetitions of each variety would be required to show a 10% difference. This would have made the experimental area too large and also in this trial it was expected that the important differences would be of a large order. Six repetitions of sixteen bush plots, involving 96 bushes of each of the eight varieties, would on the other hand fit nicely into the available ground space and would probably show a difference of about 15%. This was considered satisfactory and the plan was adopted.

(3) *The arrangement of the plots.*

The available ground was, therefore, divided into forty-eight plots each to contain sixteen bushes, there being three plots in each row, with two headland bushes (not included in the trial) at each end. The forty-eight plots were then divided into six blocks as shown. One plot of each of the eight plots within each block was planted with each of the varieties, the position of each variety within the block being determined by drawing cards at random from a pack. The resulting plan is shown in Fig. xiii.

FIG. XIII.

Plan of Black Currant Variety Trial—showing arrangement of plots in 6 randomized blocks and 1930 crop in lbs. per plot.

BLOCK I.		BLOCK III.		BLOCK V.	
Sdg. A.	28.8	Boskoop	12.5	Seabrooks	23.6
Goliath	17.0	Goliath	16.3	French	13.3
French	19.2	Sdg. B.	23.2	Sdg. A.	19.3
Sdg. C.	32.3	Seabrooks	15.1	Baldwin	35.9
Seabrooks	23.8	Sdg. C.	28.6	Sdg. B.	41.9
Baldwin	39.1	Sdg. A.	24.5	Boskoop	18.1
Boskoop	14.6	French	15.0	Goliath	23.3
Sdg. B.	35.2	Baldwin	28.3	Sdg. C.	35.6
Baldwin	37.8	Sdg. C.	35.0	Goliath	23.5
Seabrooks	22.0	Sdg. B.	40.3	Baldwin	36.0
Boskoop	15.0	Boskoop	14.2	Sdg. B.	35.6
Sdg. A.	24.5	Sdg. A.	19.7	Seabrooks	26.5
Goliath	13.1	French	15.9	Sdg. A.	16.1
Sdg. C.	29.1	Seabrooks	17.1	Sdg. C.	39.7
Sdg. B.	28.8	Goliath	10.7	Boskoop	15.0
French	14.8	Baldwin	39.5	French	22.5
BLOCK II.		BLOCK IV.		BLOCK VI.	

→ N.

Each plot consists of 16 bushes running North and South.

(d) Planting.

The experiment was planted up in February, 1928, and at planting time a record of the weight of the bushes was taken. After planting, the bushes were cut down to ground level. Accordingly no crop was obtained in 1928 and the first crop was harvested in 1929.

It was not, however, until 1930 that any considerable weight of fruit was obtained, and the figures for this crop are shown on Fig. (xiii).

(e) Harvesting.

In picking, each variety was picked separately and a gang of sixteen women and one recorder was employed. With an average of 25 lbs. of fruit per plot approximately, it took this gang between 20 and 25 minutes to pick each plot, making the cost of picking about 2/3 a plot, the women being paid by the hour.

Each plot was harvested as a whole and the weights were taken in the field in pounds, a spring balance graduated in tenths of a pound being used.

(f) Analysing the Results.

The analysis of the 1930 crop records is given here as an example. In it, for the sake of uniformity, as nearly as possible the same method and notation is employed as that used by Fisher and Wishart in the examples of agricultural experiments given in the memorandum issued by the Imperial Bureau of Soil Science (43).

In this first example it is assumed that no calculating machine is available. No aid other than tables of squares and square roots and a book of logarithms is necessary to complete the analysis.

The principal steps necessary in the analysis may be summarized as follows :—

(1) The data are tabulated in such a way that they may be easily dealt with.

(2) The deviations of each plot yield from the mean or average are found, squared and summed.

(3) The sum of these squared deviations is divided up into the parts appropriate to :

  (*a*) Differences between varieties.

  (*b*) Differences in position.

  (*c*) The random errors of the experiment.

(4) The mean squared deviation, or variance, appropriate to each source of variation is calculated.

(5) The significance of the differences between the variances due to variety, position and that due to the experimental error are tested.

(6) The average yields per treatment are tabulated and compared by means of the standard error which is obtained from the variance due to the random errors of the experiment.

The first step is to tabulate the plot yields in such a way that they can be most easily dealt with. This is done in Table (1), the yields of each being taken from Fig. (xiii) and written down so that, by summing each column, the total yield of the 6 plots of each variety, and, by summing each row, the yield of each block of 8 plots is obtained.

TABLE 1.

Actual plot yields in pounds.

	Baldwin	Boskoop	Goliath	Sea-brooks	French	Sdg. A.	Sdg. B.	Sdg. C.	Total	Mean
Block I.	39.1	14.6	17.0	23.8	19.2	28.8	35.2	32.3	210.0	
II.	37.8	15.0	13.1	22.0	14.8	24.5	28.8	29.1	185.1	
III.	28.3	12.5	16.3	15.1	15.0	24.5	23.2	28.6	163.5	
IV.	39.5	14.2	10.7	17.1	15.9	19.7	40.3	35.0	192.4	
V.	35.9	18.1	23.3	23.6	13.3	19.3	41.9	35.6	211.0	
VI.	36.0	15.0	23.5	26.5	22.5	16.1	35.6	39.7	214.9	
Total	216.6	89.4	103.9	128.1	100.7	132.9	205.0	200.3	1176.9	147.1125
Average	36.1	14.9	17.32	21.35	16.78	22.15	34.17	33.38	196.15	24.51875

From these sums the total yield of the experiment is arrived at. It is now easy to write down in the bottom row the average yield of each variety per plot, the average total yield per 6 plots for all varieties = 147.1125, the average total yield per block of 8 plots = 196.15, and the general mean per plot for the whole experiment = 24.51875.

It will be remembered that in the whole experiment there is a total amount of variation or variance that is made up of two parts, (*a*) that between plots treated alike, and (*b*) that between plots treated differently. Further (*a*) is sub-divided into (*c*) that part which is due to the inherent

variability of the material, and (*d*) that part which is due to differences in position. The variance or mean square deviation, which is usually designated as "σ^2" is obtained by dividing the sum of the squared deviations of all the observations from the mean by the number of degrees of freedom. (See page 31).

The sum of the squared deviations may be obtained straightforwardly by subtracting each observation from the mean and squaring it.

Much arithmetic may be avoided, however, if a simpler method is used. Actually the same result is obtained by squaring each observation and subtracting from the sum of these squares the product of the total and the mean, or, more simply still, the square of the total divided by the number of observations, e.g., to find the sum of the squared deviations of eight observations :

$$5,\ 6,\ 12,\ 9,\ 6,\ 3,\ 10,\ 13$$
$$\text{Total} = 64 \qquad \text{Mean} = 8$$
$$\text{Sum of Squares} = 25 + 36 + 144 + 81 + 36 + 9 + 100 + 169$$
$$= 600$$
$$\text{Total} \times \text{Mean} = \text{Total}^2 \div 8 \qquad = 512$$
$$\text{Sum of Squared deviations} = (600 - 512) = 88.$$

The first part of the analysis is, therefore, to find the total sum of squared deviations and split it up into those component parts which are appropriate to each of the three sources of variation.

The total sum of squared deviations and that part (*a*) between like plots can be most easily obtained in one operation. This calculation is carried through in Tables (2) and (3).

TABLE 2.

Calculation of the sum of the squared deviations within plots treated alike.

	Baldwin	Boskoop	Goliath	Seabrooks	French	Sdg. A.	Sdg. B.	Sdg. C.
Block I.	1528.81	213.16	289.00	566.44	368.64	829.44	1239.04	1043.29
II.	1428.84	225.00	171.61	484.00	219.04	600.25	829.44	846.81
III.	800.89	156.25	265.69	228.01	225.00	600.25	538.24	817.96
IV.	1560.25	201.64	114.49	292.41	252.81	388.09	1624.09	1225.00
V.	1288.81	327.61	542.89	556.96	176.89	372.49	1755.61	1267.36
VI.	1296.00	225.00	552.25	702.25	506.25	259.21	1267.36	1576.09
Sum of Squares	7903.60	1348.66	1935.93	2830.07	1748.63	3049.73	7253.78	6776.51
(Total)^2	46915.56	7992.36	10795.21	16409.61	10140.49	17662.41	42025.00	40120.09
$\div 6$	7819.26	1332.06	1799.2017	2734.935	1690.0817	2943.735	7004.1667	6686.6817
Sum of Squares of deviations from Mean	84.34	16.60	136.7283	95.135	58.5483	105.995	249.6133	89.8283

Using a table of squares, the square of each plot yield is written down in the same order as in the previous table and totalled in each case in row 7. Thus the sum of the squares of the plot yields for Baldwin is 7903.60, for Boskoop 1348.66 and so on. In row 8 the total yield of each set of 6 plots is squared, e.g., Baldwin $(216.6)^2 = 46915.56$, and in row 9 divided by the number of observations, in this case 6. This amount is then subtracted from the sum of squares in row 7, and the sum of the squared deviations within each set of 6 like plots is obtained. Thus for Baldwin (7903.60 — 7819.26) = 84.34.

The numbers in rows 7 and 10 are now transferred to Table (3) columns 1 and 2.

TABLE 3.

Calculation of total sum of squared deviations from the general mean and the sum of squared deviations within plots treated alike.

	Sum of Squares	Sum of Squares of deviations from variety means.
Baldwin	7903.60	84.3400
Boskoop	1348.66	16.6000
Goliath	1935.93	136.7283
Seabrooks	2830.07	95.1350
French	1748.63	58.5483
Sdg. A.	3049.73	105.9950
Sdg. B.	7253.78	249.6133
Sdg. C.	6776.51	89.8283
Sum	32846.91	836.7882
(Grand Total)^2 ..	1385093.61	
Divided by 48 ..	28856.12	
Sum of Squares of Deviations from General Mean ..	3990.79	

The sum of column 1 in Table (3) gives now the sum of squares for the whole 48 plots, and by subtracting from this the square of the total yield of all the plots (1176.9) divided by 48, the total number of observations, the sum of squared deviations from the general mean for the whole experiment is obtained, viz. :—

$$32846.91 - 28856.12 = 3990.79.$$

Now adding up column 2 the sum of the squared deviations of plots treated alike from their respective means is also obtained and is 836.79.

The next step is to calculate the sum of the squared deviations between the plots treated differently. This is most easily obtained from the variety totals in row 7 of Table (1).

TABLE 4.

Calculation of sum of squared deviations between varieties and between blocks.

Varieties.			Blocks.		
	Total.	Square.		Total.	Square.
Baldwin ..	216.6	46915.56	I.	210.0	44100.00
Boskoop ..	89.4	7992.36	II.	185.1	34262.01
Goliath ..	103.9	10795.21	III.	163.5	26732.25
Seabrooks ..	128.1	16409.61	IV.	192.4	37017.76
French ..	100.7	10140.49	V.	211.0	44521.00
Sdg. A ..	132.9	17662.41	VI.	214.9	46182.01
Sdg. B ..	205.0	42025.00			
Sdg. C ..	200.3	40120.09			
Sum		192060.73	Sum		232815.03
Divide by 6		32010.12	Divide by 8		29101.88
Subtract		28856.12	Subtract		28856.12
Sum of squared deviations ..		3154.00	Sum of squared deviations ..		245.76

The calculation is carried through on the left hand side of Table (4) in exactly the same way as in Table (3), except that, since each of the totals is the sum of 6 plots, the sum of squares 192060.73 must be divided by 6 before the product of the general mean and grand total 28856.12 is subtracted from it. The sum of squared deviations between unlike plots is therefore 3154.00.

The first part of the analysis is now complete, and can be checked, for the sum of the squared deviations between like plots (or within varieties) and that between unlike plots (or between varieties) together make up the total. Thus,

Sum of Squared deviations :—

Within Varieties	=	836.79
Between Varieties	=	3154.00
Total	=	3990.79

In the next part of the analysis the sum of the squared deviations within varieties must be split up into its component parts, i.e., that part which is due to differences in position on the ground, and that part which is due to the inherent variation in the plots or the unavoidable errors of the experiment.

That part due to position may be computed by calculating the sum of squares of the block totals from the mean of the six blocks, and this is done on the right hand side of Table (4). In this case the sum of the squares of the block yields must be divided by eight, since there are eight plots in each block.

The sum of the squared deviations between blocks is, therefore, 245.76 and the analysis of this section can be completed by subtracting this from the sum of squared deviations within varieties already found :—

Sum of Squared deviations due to position	245.76
Inherent Errors	591.03
Within Varieties	836.79

Calculation of the variance.

Degrees of Freedom.

It has been said that the variance or mean squared deviation is obtained by dividing the sum of the squared deviations by the number of degrees of freedom. The name " mean squared deviation " implies that the variance is the average of the squared deviations ; but in fact this average is obtained by dividing not by the number of observations but by the number of independent comparisons or " degrees of freedom." In this case this number is one less than the number of observations.

The full analysis can now be completed and this is done in Table (5).

TABLE 5.

Analysis of variance.

Variance due to	Degrees of Freedom.	Sum of Squares of Deviations.	Variance (σ^2)	$\frac{1}{2}\log_e \sigma^2$
Blocks ...	5	245.76	49.152	1.9474
Varieties ...	7	3154.00	450.57	3.0553
Error ...	35	591.03	16.886	1.4133
Total ...	47	3990.79	84.910	

Standard Deviation (σ) $= \sqrt{16.886} = 4.109$

Standard Error of the Mean of 6 plots $= \sqrt{\frac{\sigma^2}{6}} = 1.6776.$

Standard Error of the Difference between two Means

$$= \sqrt{\frac{2\sigma^2}{6}} = 2.372$$

Significant Difference $= 2.372 \times 2.228$
$= 5.284.$

In the second column are the number of degrees of freedom corresponding to the different components of the total variance in column 1. Thus there are 48 plots in the experiment and therefore 47 degrees of freedom. This is made up of 5 for the variation between blocks, since there are 6 blocks, 7 for that between varieties, there being 8 varieties, leaving 35 to be allotted to the errors of the experiment.

In the next column are written the sum of the squared deviations in each case, and to find the mean square deviation or variance, column 3 is divided by column 2. Thus for varieties the sum of the squared deviations is 3154.00, and there being seven degrees of freedom, the variance is 450.57. Similarly that for blocks is 49.152 and for Error 16.886.

Now it has been already pointed out that if the variance contributed by the differences between varieties is much greater than that due to the inherent variability of the material, then there must be significant differences between the yields of the varieties under trial. It is, therefore, now necessary to test the significance of the difference between the variance due to variety, i.e., 450.57 and that due to Error, i.e., 16.886. This is done by applying what is known as the "z" test and is carried out as follows. In column 5 is written down half the natural logarithm of each variance.

The easiest way to do this is to look up the logarithm to the base ten and multiply by 1.15129, which is half the natural logarithm of 10.

For example the variance due to variety is 450.6.

$$\begin{aligned} \log_{10} 450.6 &= 2.6538 \\ \tfrac{1}{2} \log_e 450.6 &= 2.6538 \times 1.15129 \\ &= 3.0553 \end{aligned}$$

The difference between the entries in column 5 is z, e.g., comparing the variance due to variety with that due to Error, the difference between the two entries is 1.6420 and this is z.

TABLE 6.

Significance of a difference in variance.

	$\frac{1}{2} \log_e \sigma^2$		$\frac{1}{2} \log_e \sigma^2$
Blocks ..	1.9474	Treatment ..	3.0553
Error ..	1.4133	Error ..	1.4133
Difference "z"	.5341	Difference "z"	1.6420
for P = .05, z = .46 approx.		for P = .05, z = .42 approx.	

Now Fisher (42) has given tables showing the normal expectation of z for levels of significance P = .05 (odds 20 to 1) and P = .01 (odds 100 to 1), for varying values of n_1, and n_2 where n_1 is equal to the number of degrees of freedom corresponding to the larger variance and n_2 equal to the degrees of freedom for the smaller. The figure already obtained for z is now compared with the corresponding value in this table. In this case the variance due to variety is much larger, so that $n_1 = 7$ and $n_2 = 35$. Consulting the table for P = .05, z is found to be about 0.42 and for P = .01 about 0.60. The value found is 1.64 and is much greater than this, so that the odds are considerably more than a hundred to one against such a difference in variance occurring by chance, and the differences in variety are significant.

In a similar way the positional variance may be tested and it is found that, though not large, there are significant differences between the six blocks, the odds being greater than 20 to 1, but not as great as 100 to 1.

The next step is to tabulate the means of the different varieties in the trial (Table 7), and thence discover which are the differences which are significant. For this test it is necessary to work out the standard error of the mean of six plots and thence the standard error of the difference between the two such means.

TABLE 7.

	Baldwin	Sdg. B.	Sdg. C.	Sdg. A.	Seabrooks	Goliath	French	Boskoop	General Mean	Standard Error	Significant Difference
Mean per plot in lbs. ...	36.1	34.2	33.4	22.1	21.3	17.3	16.8	14.9	24.5	1.68	5.28
Per cent. ...	147	140	136	90	87	71	69	61	100	6.9	21.6

The standard error of the mean of six plots is found by dividing the variance due to Error (16.886) by six and taking the square root (see bottom of Table 5). This is found to be 1.68. The standard error of the difference is obtained by finding the square root of *twice* the variance due to Error divided by six, and is 2.372.

Now, in order to test each individual difference in yield, it is necessary to divide the difference between the means by the standard error of the difference : thus, the difference between Seedlings C. and A. is 11.3 and this divided by 2.372 is 4.76. This figure is usually known as "*t*." Fisher has given a table showing the normal expectation of *t* for various numbers of observations and levels of significance (42). In this table the appropriate figure for *n* is found by adding the number of observations in each set and subtracting two. In this case there are six plots in each case, so that *n* is equal to 10.

For a difference to be significant, it is usually considered sufficient if the odds are twenty to one against the difference being due to chance or in other words for P = .05.

Consulting the table, the value of *t* for P = .05 and *n* = 10 is 2.228. The value obtained is much greater than this, being 4.76, so that there is no doubt about the significance of that difference.

Since the number of observations is the same in each case, it is only necessary to calculate a single "significant difference" for any pair of varieties, by multiplying the standard error of the difference by the appropriate figure for "*t*." In this case the significant difference is thus found to be $2.228 \times 2.372 = 5.28$. Now any difference which is greater than this is significant.

Examining Table 7 it is at once obvious that the varieties fall into two distinct groups, i.e., Baldwin, Seedlings B and C, and the remainder. The three varieties in the upper group cannot be said to have differed significantly in their crop in 1930 ; while the only definite difference in the lower group is that between Seedling A. and Boskoop.

The table is usually completed by presenting each average as a percentage of the general mean for the sake of easy comparison.

It may be noted that in this year it has been possible to prove a difference of 21.6%. As the experiment was designed to show a 15% difference when the bushes were in full cropping, this is by no means a bad result for the first year.

2. Strawberry Strain and Time of Planting Trial.

This experiment is selected as an example of the two factor trial ; that is to say two distinct issues are under observation in the same experiment.

(a) The Project.

The object of the trial was to test, firstly three different strains of the variety Royal Sovereign —designated A, B and C—for cropping, general health and vigour. Secondly, the time of planting may be of great importance in strawberry growing ; and in this experiment—the second of a series—four different times for planting were selected. In the original project the months of August, October, December and February were chosen. However, a very large proportion of the August plants were killed by drought immediately following planting, so that these plots had to be scrapped and were replaced by an April planting. Also, hard frost prevented the February planting, so that the actual times of planting owing to the exigencies of season were finally October, December, March and April. (93).

Each strain was, therefore, planted at the four different times of year, so that there were in all twelve differently treated sets of plots, e.g. :—

Strain	A	planted in	October	Strain	B	planted in	October	Strain	C	planted in	October
,,	A	,,	December	,,	B	,,	December	,,	C	,,	December
,,	A	,,	March	,,	B	,,	March	,,	C	,,	March
,,	A	,,	April	,,	B	,,	April	,,	C	,,	April.

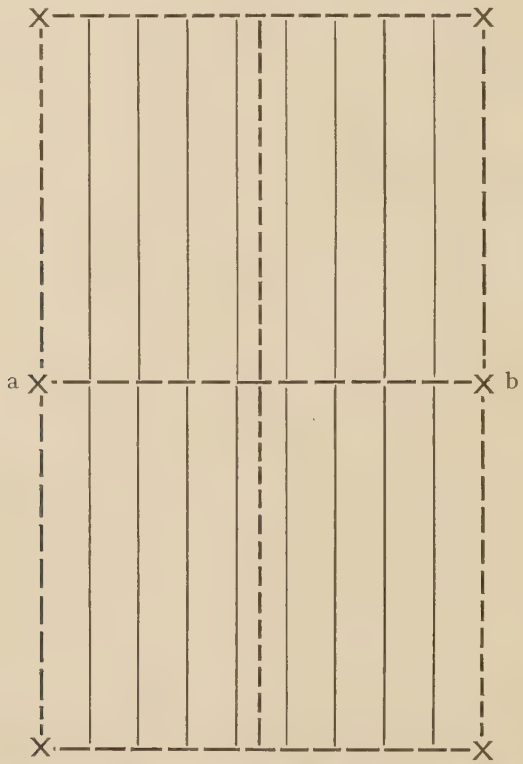
(b) Raising the Material.

The same care is necessary in raising the runners for planting as with other horticultural material. The plants from which the runners were produced were carefully observed through the season, all diseased or weak plants being eliminated. Only one year old plants were used as parents, and the number of runners to a plant was restricted. Only strong well-rooted runners were selected for planting.

(c) Size of Plot and Number of Replications.

At the time of planning this experiment, nothing definite was known about the variation in strawberries. This present trial was the first in which carefully selected plants were used, concerning the variation of which nothing was known. The plots were, therefore, selected of a convenient size and shape, rather on the large side, though not as large as in the previous trial. Being planted between the rows of a young plum and cherry orchard, each plot was 78 sq. yds., or .016 of an acre in area, and consisted of four rows each containing 47 plants, 15 inches apart within the rows, and three feet between the rows :—

FIG. XIV.



NOTE: *Continuous lines represent Strawberry rows, and dotted lines plot boundaries. Each tree is represented by a cross.*

It will be noticed from Fig. xiv. that some of the boundaries of the strawberry plots coincide with the tree rows (e.g., from a to b). This is definitely a mistake, which should be avoided wherever temporary experiments are done in young orchards of permanent trees, since at picking time paths are bound to be made between the strawberry plots and this may easily result in damage to the trees. Such trials are now designed so that the boundaries of the temporary plots lie between the tree rows as far as possible.

In this trial, each of the twelve individual treatments was repeated four times, there being 48 plots in all. The advantage of the two factor trial is now apparent ; for, although there are only four replications of each combination of strain and time of planting, since each strain is planted in four different months, there are sixteen plots devoted to each of the three strains ; and, since in each of the four months three different strains are planted, there must be twelve plots planted in each month for comparison.

Since this trial was laid out, a uniformity trial has been in progress and much more is now known of the variability of such material. Although, until this uniformity trial is completed, it will not be possible to lay down any definite number of replications, at the present time rather more repetitions of smaller plots of about 100 plants are being used. This size of plot is considered to be the minimum under Malling conditions ; since, in plots smaller than this, the errors in harvesting a crop, which may have to be picked over six or eight times, become unduly important.

(d) Planning.

The planning of this trial was done in exactly the same way as in the previous example. The method of randomized blocks was used again, and in this case there were four blocks each containing twelve plots. A plan of the trial is shown in Fig. xv.

FIG. XV.

Strawberry—Strain and Time of Planting Trial (showing 1930 crop in lbs. per plot). Method : 4 Randomized blocks.

B October 55.9	C December 52.7	A October 63.5	C April 35.5	C October 54.0	B March 43.9	B April 38.8	B October 43.4
B December 24.0	A April 30.0	A December 40.5	B March 30.5	A March 38.9	A April 33.3	A October 50.6	C April 31.4
C March 32.7	B April 27.5	C October 50.4	A March 38.9	B December 23.6	A December 47.9	C December 38.0	C March 37.1
A March 37.0	A December 36.7	C October 50.8	B December 39.0	B March 34.0	B April 27.5	A December 42.2	C October 48.2
C April 29.5	B April 31.2	A April 27.5	C December 43.3	A October 46.6	C December 27.2	A March 31.5	C March 40.1
A October 47.2	B October 47.8	C March 34.6	B March 35.0	C April 25.7	A April 26.1	B October 45.3	B December 31.0

(e) Planting.

Strawberries are subject to a number of pests and diseases, and in addition to the careful selection of apparently healthy plants, it may be necessary to fumigate or dip the plants in order to kill any insects which may be present.

Planting this trial has been comparatively simple, as only twelve plots have to be planted at a time ; even so the greatest care has been needed to see that the plots are planted in the right positions. It is only necessary therefore to label each of the plots to be planted on each particular day, before planting is begun. It is better to plant only one strain at a time, using all the available labour for each plot to avoid personal bias. Six men were able to lift the runners and plant the twelve plots fairly easily in one day.

(f) Records during the First Year.

At the present time, it is still a matter for argument, whether strawberries grown commercially should be allowed to crop in their first year or not.

From the experimenter's point of view the earlier a result can be obtained the better, and that is one reason for allowing a crop. In addition, in this particular trial, while it might have been a paying proposition to allow the autumn planted plots to crop, the reverse might have been the case, where spring planting was adopted. A crop was therefore harvested in the first season.

During this year records of vigour were expected to be of great value and accordingly two records of spread (p. 21) were taken at the beginning and end of the season.

A record of the runners made by the plants was also obtained when these were of necessity removed.

Theoretically, of course, the runners should never be allowed to develop, but this would entail going over the plots very frequently and in practice they are generally removed two or three times during the season.

(g) Harvesting.

The first full crop was harvested in the second year. Referring to the plan (Fig. xiv.) it will be remembered that each plot consists of four rows and that the rows run North and South.

A start was made at the south end of the field and two women were allotted to each plot—thus, there being eight plots across the field from east to west, 16 women were needed.

Each pair of women then picked along the rows from south to north, stopping when they came to the end of a plot and leaving their baskets at the junction, until they had picked the whole six plots in their beat. A recorder followed the gang weighing the produce of each plot. At the height of the season it took a gang such as this about an hour to pick over the whole experiment.

The produce was weighed as usual in pounds per plot. The total crop in 1930 for each plot is shown in Fig. xv.

(h) Analysis of the Results.

In this example the method recommended by Fisher and Wishart, of subtracting a working mean somewhere in the neighbourhood of the true mean from each observation, before working out the sum of squares of deviations, is adopted. By this means a great deal of labour in computation is avoided. To take once more a simple example, to find the sum of squares of deviations of four observations in the ordinary way entails a considerable amount of labour, e.g. :—

	Observation	Square
	107	11449
	96	9216
	111	12321
	82	6724
Total	396	39710
Total$^2 \div 4$		39204
Sum of squares of deviations		506

The mean of the four observations is about 100 (actually 99) so that 100 can be subtracted from each observation and the calculation simplified :—

	Observation	Square
	+7	49
	−4	16
	+11	121
	−18	324
Total	−4	510
Total² ÷ 4		4
Sum of squares of deviations		506

In the present example, a brief examination of the data reveals that the general mean for the 48 plots must be somewhere about 40 ; this is, therefore, taken as the working mean and subtracted from the yield of each plot, before these yields are tabulated for analysis in Table (8). Thus the yield of the A. October plot in Block 1 is 63.5 and this is tabulated in Table (8) as 23.5.

TABLE 8.

	Treatment.												
Block	A Oct.	A Dec.	A Mar.	A Apr.	B Oct.	B Dec.	B Mar.	B Apr.	C Oct.	C Dec.	C Mar.	C Apr.	Total.
I.	23.5	0.5	−1.1	−10.0	15.9	−16.0	−9.5	−12.5	10.4	12.7	−7.3	−4.5	+2.1
II.	10.6	7.9	−1.1	−6.7	3.4	−16.4	3.9	−1.2	14.0	−2.0	−2.9	−8.6	+0.9
III.	7.2	−3.3	−3.0	−12.5	7.8	−1.0	−5.0	−8.8	10.8	3.3	−5 4	−10.5	−20.4
IV.	6.6	2.2	−8.5	−13.9	5.3	−9.0	−6.0	−12.5	8.2	−12.8	0.1	−14.3	−54.6
Total	47.9	7.3	−13.7	−43.1	32.4	−42.4	−16.6	−35.0	43.4	1.2	−15.5	−37.9	−72.0
Mean	11.975	1.825	−3.425	−10.775	8.1	−10.6	−4.15	−8.75	10.85	0.3	−3.875	−9.475	

Total	= −72.0	A	= −1.6	Oct.	= 123.7
General Mean	= −1.5	B	= −61.6	Dec.	= −33.9
		C	= −8.8	Mar.	= −45.8
				Apr.	= −116.0
				Total	−72.0

The totals for the four plots devoted to each treatment and the twelve plots in each block are then obtained by adding the columns and rows of the table as in the previous example. The totals for each set of four plots treated alike are then transferred to Table (9) column 2.

TABLE 9.

Treatment.	Total (4 plots)	Sum of squares of individual Plot yields.	(Total)²	(Total)² ÷ 4	Sum of squares of Deviations from Variety means.
A. October ..	47.9	760.01	2294.41	573.6025	186.4075
A. December ..	7.3	78.39	53.29	13.3225	65.0675
A. March ..	−13.7	83.67	187.69	46.9225	36.7475
A. April ..	−43.1	494.35	1857.61	464.4025	29.9475
B. October ..	32.4	353.30	1049.76	262.44	90.86
B. December ..	−42.4	606.96	1797.76	449.44	157.52
B. March ..	−16.6	166.46	275.56	68.89	97.57
B. April ..	−35.0	391.38	1225.00	306.25	85.13
C. October ..	43.4	488.04	1883.56	470.89	17.15
C. December ..	1.2	340.02	1.44	0.36	339.66
C. March ..	−15.5	90.87	240.25	60.0625	30.8075
C. April ..	−37.9	408.95	1436.41	359.1025	49.8475
Total ..	−72.0	4262.40	12302.74		1186.715
Divide col 4. by 4 ..			3075.685		
Total² ÷48 ..		108.00	108.00		
Sum of Squares of Deviations ..		4154.40	2967.685		

The next step is to calculate the sums of squares of the individual plot yields and this is done as in the previous example (but omitted here for the sake of brevity) and the sums transferred to Table (9) column 3. Thus the four A. October plots had a total yield of 47.9 and the sum of their squares is 760.01.

In this table, the whole of the first part of the analysis can be conveniently carried out. Adding up column 2, the total of the plot yields is found to be —72.0. Next, column 3 is summed and this gives the sum of squares of all the 48 plots, i.e., 4262.40. The square of the total divided by the number of plots, i.e., $(-72)^2 \div 48$, is 108.0 and this being subtracted from the sum of squares gives the sum of the squared deviations of all the plot yields from the general mean, i.e. 4154.40 This has now to be split up into the two parts corresponding to the variation between plots treated alike (or within treatments) and that between plots treated differently (i.e., between treatments).

To find the sum of squared deviations within treatments the square of the total of each set of four plots (column 2) divided by the number of plots (i.e., four) has to be subtracted from the appropriate sum of squares.

Thus for A. October.

Sum of squares	=	760.01	(col. 3).
$\text{Total}^2 \div 4 = (47.9)^2 \div 4$	=	573.6025	(col. 5).
Sum of squared deviations	=	186.4075	(col. 6).

The total of column 6 is the sum of squared deviations within treatments, i.e., 1186.715.

The sum of squared deviations between treatments is computed in column 4. In this column are written down the squares of the totals (e.g., $47.9^2 = 2294.41$), these, being added up and making 12302.74, must be divided by four, since each total is the sum of four plots. From this is subtracted, as before, the grand total squared and divided by 48, i.e., 108, when the sum of squared deviations between treatments is found to be 2967.685. The first part of the analysis is now complete :—

Sum of squared deviations :		
(*a*) Within Treatments	=	1186.715
(*b*) Between Treatments	=	2967.685
Total		4154.40

The second step in the analysis is to split up the part (*a*) into its component parts ; and this is done as before by finding the sum of squared deviations between blocks and subtracting from that within treatments. This is computed, in the same way as in the preceding example, on the left hand side of Table (10).

TABLE 10.

Between Blocks.			Between Strains.			Between Times.		
Block	Total (12 plots)	Square.	Strain.	Total (16 plots)	Square.	Time.	Total (12 plots)	Square.
I. ..	+2.1	4.41	A ..	−1.6	2.56	October ..	123.7	15301.69
II. ..	+0.9	0.81	B ..	−61.6	3794.56	December	−33.9	1149.21
III. ..	−20.4	416.16	C ..	−8.8	77.44	March ..	−45.8	2097.64
IV. ..	−54.6	2981.16				April ..	−116.0	13456.00
Total ..	−72.0	3402.54	Total ..	−72.0	3874.56	Total ..	−72.0	32004.54
Divide by 12		283.545	Div. by 16..		242.16	Divide by 12		2667.045
$\text{Total}^2 \div 48$		108.0	$\text{Total}^2 \div 48$		108.00	$\text{Total}^2 \div 48$		108.00
Sum of Squares of Deviations		175.545	Sum of Squares of Deviations		134.16	Sum of Squares of Deviations		2559.045

In this case there are twelve plots in each block, so that the sum of squares must be divided by 12. The sum of squared deviations due to position is thus found to be 175.545 and this part of the analysis may be completed :—

Sum of squared deviations		
Within treatments	=	1186.715
Between Blocks	=	175.545
Due to error	=	1011.170

The third step is to split up the part (*b*) " between treatments " into its component parts, i.e., that part which is contributed (i) by differences in strain, (ii) by differences in time of planting, (iii) by the interaction of strain and time of planting ; (i) and (ii) are computed in Table (10) in exactly the same way as the part between blocks. Thus the total yield for the 16 plots containing Strain A (after subtracting the working mean of 40 from each plot yield), is —1.6, B —61.6 and C —8.8 ; and the sum of squared deviations is computed in the ordinary way and found to be 134.16.

Similarly the total yield of the 12 plots planted in October is 123.7, in December —33.9, in March —45.8 and in April —116.0 and the sum of the squared deviations is computed as 2559.045.

The amount due to any differential effect can now be found by subtraction :—

Sum of squared deviations		
Between Treatments	=	2967.685
Due to Strain	=	134.16
Due to Time of Planting	=	2559.045
Due to differential effect	=	274.48

The complete analysis of variance is now carried out in Table (11).

TABLE 11.

Analysis of Variance.

Due to	Degrees of Freedom.	Sum of Squares of Deviations.	Mean Square (σ^2)	$\frac{1}{2}\log_e$
Position	3	175.5	58.5	2.0345
Treatment :				
Strain	2	134.2	67.1	2.1031
Time	3	2559.0	853.0	3.3744
Strain and Time ..	6	274.5	45.75	1.9116
Total Treatment ..	11	2967.7	269.8	
Error	33	1011.2	30.642	1.7112
Total	47	4154.4	88.391	

In column 2 the degrees of freedom are apportioned in the following way. Since there are 48 plots there must be 47 degrees of freedom in all ; these are divided in the first place into 3 for position, since there are 4 blocks, 11 for treatment, since there are twelve differently treated sets of plots in all, and 33 for error. Of the eleven degrees of freedom for treatment, there are two appropriate to strain and three to time of planting (since there are 3 strains planted at 4 different times), while the remaining six are allotted to differential effect.

The sums of squared deviations are now written down in column 3 and, if these are divided by the appropriate degrees of freedom, the mean square or variance is obtained in column 4.

TABLE 12.

½ log$_e$	½ log$_e$	½ log$_e$
Position 2.0345 Error 1.7112	Strain 2.1031 Error 1.7112	Time 3.3744 Error 1.7112
" z " .3233	" z " .3919	" z " 1.6632
"z" for P = .05 = .53 approx.	.595 approx.	0.53 approx.

Standard Deviation (σ) = 5.536

Coefficient of variability $\frac{(100\sigma)}{38.5}$ = 14.4%

Standard Error of the Mean of 4 plots = 2.77
" " " 12 " = 1.60
" " " 16 " = 1.38

The " z " test is now applied as before in column 5 of Table (11) and Table (12) to decide which of the treatments have given significant differences. Thus " z " for position and error is 0.3233 and the appropriate " z " for P = .05, $n_1 = 3$ and $n_2 = 33$, is about 0.53 ; so that positional differences are not significant. In the same way there is no significant effect of strain or in the interaction of strain and time. Planting at different times of the year has, on the other hand, made a great difference to the yields, and one which is highly significant, z being 1.66.

It is now only necessary to tabulate the average yields for each treatment, to see which are the differences which have caused this result. This is usually done in three tables.

TABLE 13 (i).

	A Oct.	A Dec.	A Mar.	A Apr.	B Oct.	B Dec.	B Mar.	B Apr	C Oct.	C Dec.	C Mar.	C Apr.	G'neral Mean	S E. of Mean of 4 plots.	Significant Difference
Mean per plot in lbs.	52.0	41.8	36.6	29.2	48.1	29.4	35.8	31.2	50.8	40.3	36.1	30.5	38.5	2.77	9.6
Cwts. per acre ..	29	23	20	16	27	16	20	17	28	22	20	17	21.6	1.5	5.3
Per cent. ..	135	109	95	76	125	76	93	81	132	105	94	79	100	7.2	24.9

In Table 13 (i) are set out the average yields for each individual treatment. These are obtained easily by adding the working mean of 40, previously subtracted from each plot yield, to each of the treatment means found in Table (8). Thus for A. October the mean yield is (40 + 11.975) or 52.0 approximately. The general mean is 38.5 and the standard error of each of these means of four plots, found from the variance due to error is $\sqrt{\frac{30.642}{4}}$ or 2.77. The standard error of a difference between any two of these means is $\sqrt{\frac{2 \times 30.642}{4}}$ or 3.91. Now looking up the table of " t " as before, for P = .05 and n = 6, i.e., (3 + 3), t is found to be 2.447. Thus the significant difference is (3.91 × 2.447) which is approximately 9.6. The yields are then presented as a percentage of the general mean and in this case the yield in cwts. per acre is added. This figure is obtained by dividing by the size of plot (i.e., .016 acre) to get the acreage yields in pounds, and reducing the result to cwt. This is most easily done by multiplying each yield in pounds by $\frac{1}{.016 \times 112}$ = 0.56 approximately.

TABLE 13 (ii).

Strain.	A	B	C	General Mean.	S.E. of Mean of 16 plots.	Significant Difference.
Mean per plot in lbs. ..	39.9	36.1	39.4	38.5	1.38	4.0
Cwts. per acre	22.2	20.2	22.1	21.6	0.8	2.2
Per cent.	104	94	102	100	3.6	10.4

In Table (13) (ii) the average yields of the sixteen plots devoted to each of the three strains are set out and compared by means of their standard error. Here the standard error is that of the mean of 16 plots so that it is $\sqrt{\frac{30.642}{16}}$ which is 1.38, and the standard error of the difference is 1.95. The table of t is now entered with n = 30, (15 + 15) and the appropriate value is 2.042. The significant difference is thus 4.0 approximately.

TABLE 13 (iii).

Time.	October.	December.	March.	April.	General Mean.	S.E. of Mean of 12 plots.	Significant Difference.
Mean per plot in lbs.	50.3	37.2	36.2	30.3	38.5	1.6	4.7
Cwts. per acre ..	28.2	20.7	20.2	16.9	21.6	0.9	2.6
Per cent. ..	131	97	94	79	100	4.2	12.2

The mean yields for the different times of planting are set out in Table 13 (iii) in exactly the same way.

In this case there are twelve plots, the standard error is 1.6 and the significant difference 4.7.

An examination of the three tables now shows that by far the most important factor in the experiment is time of planting; and that, while the yields from the plots planted in December and March do not differ significantly, the October planted plots have invariably yielded the highest crop and the April planted plots the least, with the curious exception of strain B planted in December. This differential effect, though not significant, is suggestive and worth looking into.

Finally it may be observed that the coefficient of variability (see footnote p. 6) of this experiment is 14.4% of the general mean and that it is possible in this case to prove a difference of 25% between the means of four plots, a satisfactory result, considering the inadequate information available at the time of planning.

3. An Apple Manurial Trial.

The two previous examples have shown the straightforward use of the randomized block method of layout applied to small fruit experiments.

For a detailed description of more complex experiments and of the latin square, the reader is referred to the memorandum of the Imperial Bureau of Soil Science by Fisher and Wishart (43).

Reference has already been made in this paper (page 16) to special modifications introduced into the randomized block method, in laying out experiments with plums and cherries; and the writer proposes now to describe in some detail a third experiment on apple trees which may be of interest as showing most of the difficulties normally met with in the case of tree experiments.

The detailed analysis of the experiments cannot be given, as it is only now, after three years spent in preparation of material and ground, that the plots are being planted up. It may, however, be pointed out that the principle is exactly the same as that already described in the two factor trial with but slight modifications in detail, which will be briefly commented upon.

(a) The Project.

(1) *The Fertilizers.*

As has already been noted, many fertilizer trials on apple trees have been attempted in the past and, with few exceptions, the results have been unsatisfactory in the extreme. The reasons for failure have been enlarged upon in an earlier section of this paper. But more recent work at Long Ashton and East Malling has shown that, under some circumstances at any rate, apple trees do respond to manuring. Wallace's work showing that on many soils potash applications are necessary (109, 110, 111) has been confirmed by a field trial carried out by Grubb (48); and a further trial at East Malling has shown the enormous difference at 10 years old between trees receiving balanced manurial treatment and others grown under starved conditions (1).

The part played by the other principal manurial constituents is, however, imperfectly understood, and it was decided accordingly to narrow the issue by confining attention to the three main constituents (nitrogen, potash and phosphate) applied in artificial form. In order to get over the difficulty of a possible lack of humus it is proposed to prepare a mixen of rotted straw (using a minimum of nitrogen for the purpose), so that this may be applied in equal weights to every plot as required.

Since the interaction of the three constituents will be in all probability the most interesting result of the trial, they are to be applied in all possible combinations. There will thus be seven sets of plots receiving different manurial treatment and one control. The eight treatments will be as follows :—

(1) No manure.
(2) Nitrogen alone.
(3) Potash alone.
(4) Phosphate alone.
(5) Nitrogen and Potash.
(6) Nitrogen and Phosphate.
(7) Potash and Phosphate.
(8) Nitrogen, Potash and Phosphate.

The fertilizers to be used will be sulphate of ammonia, sulphate of potash and superphosphate and will be applied at standard rates.

(2) *The Trees.*

Such a manurial trial would, if confined to but one combination of stock and scion, necessarily be very incomplete, since it is already known, not only that varieties respond differently to applications of a complete fertilizer as compared with starvation, but also that the response of the same variety is markedly changed if the rootstock is varied (1).

Two contrasting varieties, and representatives of the four main groups of rootstock have, therefore, been included in the trial. The varieties chosen are Cox's Orange Pippin and Beauty of Bath, and these have been "worked" on the following stocks, the very vigorous free stock Malling No. XII, the vigorous Broad-leaved English Paradise (Malling No. I), the semi-dwarfing Doucin Amélioré (No. V), a stock very liable to induce leaf-scorched trees (56), and the dwarfing Jaune de Metz (No. IX).

(b) Planning the Experiment.

That this project is one of large proportions is at once obvious, since with eight different manurial treatments and eight kinds of tree there must be 64 combinations of manure, rootstock and scion. The size of plot is, therefore, the first question to answer before any method of arrangement can be selected.

(1) *Size of Plot.*

It has been emphasized before that the individual tree is the only logical horticultural equivalent of the agricultural plot. In a manurial trial it is essential, however, that each differently manured plot should be effectively separated from the next, especially when the wide root range of apple trees is taken into consideration (92). To do this, it is necessary to have a row of buffer trees between plots receiving different manurial treatment. Thus, if individual trees

were treated in exactly the same way as ordinary plots, only one tree in four could be used for the trial. Further, sixty-four different kinds of tree, even if only repeated four times, would entail the use of 1,024 trees, of which only 256 would be experimental ; and if these were all distributed at random, the experiment would be not only uneconomical, but also more or less impossible to manage in practice.

Moreover, it is well known that the size of the trees on the different rootstocks varies, and while those on No. IX can safely be planted 10 foot square, those on No. XII eventually need a much greater space.

In order to make the trial a practicable possibility, it seemed essential therefore that some form of systematic arrangement should be employed and that the manurial plots should be larger than one tree.

It was accordingly decided to sacrifice some of the possible results of the experiment, and a " plot " for manurial purposes has been devised, in which are represented seven of the eight different kinds of tree proposed ; and these plots of trees, which are always the same, are distributed in exactly the same way, as if the trial were a simple manurial trial on strawberries or any other crop.

The composition of this plot is shown in Fig. xvi. It consists of nine trees planted 10 ft. square and is separated from adjacent plots by a double guard row—the space between the guard rows being left entirely unmanured. The nine trees are arranged as follows : In the centre is one tree of Cox's Orange Pippin on the very vigorous No. XII. The two varieties on numbers I and V are planted at the four corners, and interplanted between these are two trees of each variety on number IX. These last will be removed when the trees become too close, then the more vigorous trees on numbers I and V, leaving in the end the presumably most long-lived tree of Cox's Orange Pippin on No. XII.

The guard rows consist of trees of two varieties, Duchess Favourite and Worcester Pearmain, on number IX, selected as being good pollenizers for the varieties in the trial.

(2) *Method of Experimentation.*

The plot being standardized, the trial can now be regarded as a simple manurial trial, and the differently manured plots are distributed in randomized blocks as before, each block containing eight plots (Fig. xvi).

Although for the purposes of arrangement each plot is treated as a composite whole, the plots will be split up when the trial comes to be analysed, and the different kinds of tree considered separately. Thus, the experiment will be treated as though seven different manurial trials had been carried out on seven different combinations of stock and scion, but that, for the sake of convenience, all seven trials had been done on one piece of ground. It will, therefore, not be possible to measure accurately the effects of the interactions of scion, stock and manure, although a very good indication of those effects will be obtained and will afford a basis for future experiments of a more intimate nature.

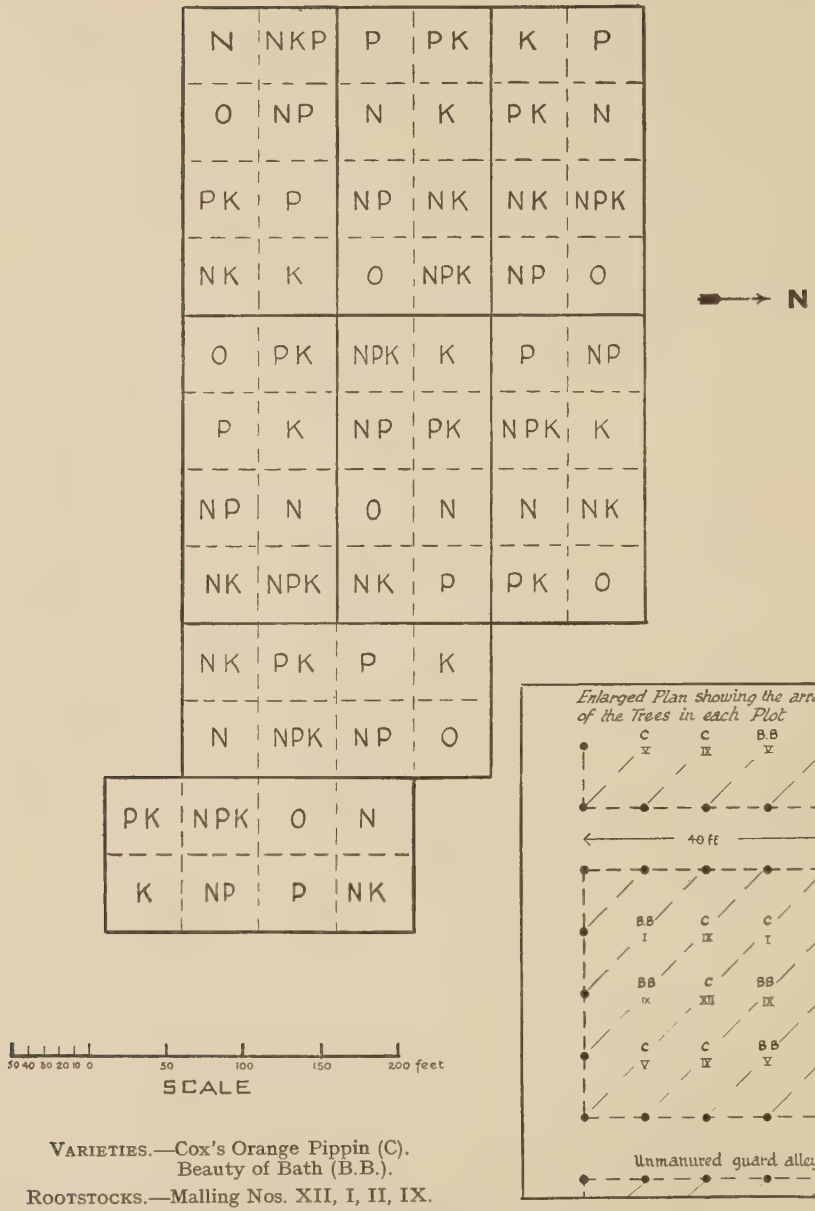
The unit will be a single tree in five cases and two trees in the other two.

(3) *Number of Replications.*

Having decided upon the unit, the question now arises as to how many times each of the differently manured plots should be repeated. A considerable body of data has been accumulated in recent years, which shows the amount of variation to be expected in apple trees on standardized stocks. In a large rootstock experiment at East Malling, reported on by Hatton in 1925 (58) and recently (61) the variation coefficient for most characters has been somewhere in the neighbourhood of 25%. Two considerations lead to the belief that yet more even trees are now a practical proposition. In the first place, in all trials so far carried out, no attempt has been made to eliminate soil variation in estimating the variability of the trees. Secondly, when the trees now mature were planted, the supply of standardized rootstocks was very limited and all available material had to be used. It seems likely that with the greater care and closer grading of both stocks and trees now practised, even less variable trees will be obtained. The probable variation coefficient for individual trees can, therefore, safely be reduced to 20%. Reference to Appendix B will show that with such variation eight replications are necessary to prove a 20% difference between the means of two sets of differently treated trees.

AN APPLE MANURIAL TRIAL.

FIG. XVI.



VARIETIES.—Cox's Orange Pippin (C).
Beauty of Bath (B.B.).

ROOTSTOCKS.—Malling Nos. XII, I, II, IX.

MANURES.—O=Nil. N=Sulphate of Ammonia.
K=Sulphate of Potash. P=Superphosphate.

This number of replications has accordingly been adopted and the plots laid out in eight randomized blocks, there being therefore 64 plots altogether. It must be remembered further that, when the differences between plots which receive one constituent and those which do not are considered, thirty-two trees will be available in each case. For example there are 32 trees of Cox's Orange Pippin on No. XII which receive nitrogen and 32 which do not. It should, therefore, be possible to prove much smaller differences between direct contrasts such as this.

(c) Raising the Material.

The preparation of the trees for experimental purposes needs the greatest care and all possible sources of variation should be eliminated as far as possible. The first point to be observed, therefore, was to raise an adequate number of trees, so that those which for any reason were not comparable, i.e., uneven in health or vigour, etc., could be rejected. It is usual at East Malling to bud at least twice as many rootstocks as there are trees required, and this was done in this case.

Although there is no evidence to prove that the size of stock at budding time is in any way correlated with the subsequent performance of the tree, this possibility must be kept in mind as long as the point is debatable. The stocks available were, therefore, graded before planting into five categories by weight, and of these the largest and smallest grades discarded. The three selected grades were then planted for budding separately.

When budding the greatest care was taken to ensure uniformity of procedure. Roberts has suggested that the position of the bud on the stock stem may be all important (90), and this and similar points were consequently kept in mind.

Great care was taken during the nursery period to keep the trees clean and free from injury. Records were taken of the growth made and the girth of stem right from the first year, and, where they occurred, any fruit buds which formed were noted. The value of these early records in interpreting the tree's ultimate performance cannot be overstressed.

(d) Selecting the Trees for Planting.

The trees were planted out at two years old. All nursery records having been taken, the available trees were carefully examined and any which were diseased or damaged, or had been checked in growth and restarted, eliminated.

The trees can be selected for planting either by weight or by their measurements above ground. Since selection by weight involves delay after lifting from the nursery, the latter course is generally the more convenient and is being adopted here although it is a moot point which is the better way.

The young trees were thus graded into categories according to their growth, and those trees nearest the average *for the particular combination of stock and scion* selected.

(e) Planting.

All the foregoing operations were completed before the trees were lifted for planting, and the trees were then lifted and weighed. The actual scheme of planting is a matter of organization which is best left to the experimenter, but one or two points are worthy of note. To ensure identification, each tree selected for planting should be given a number in the nursery and the tree labelled with this number prior to being lifted. The trees should be allotted to their final positions at random.

Before planting a manurial trial, it is very necessary to know something of the soil. In the experiment under discussion the land has been cropped evenly with cereals for the last four years, and harvesting in small plots has given valuable information concerning the soil variation as shown by its effect upon these crops. In addition borings have been made at intervals over the ground and soil samples taken at varying depths.

The further records which will be taken on these experiments will be on the lines given previously in this paper and need not be described here.

(f) Analysing the Results.

Since the trial is only now beginning, there are no data to analyse. A word, however, may be said as to the procedure. Each different kind of tree will be considered separately and the

experiment accordingly will resolve itself into a straightforward manurial trial with three factors. A study of the degrees of freedom will indicate the lines on which the analysis will proceed.

There being 64 plots in each case, there will be 63 degrees of freedom altogether. These will be divided into : seven for position, seven for treatment, there being eight blocks and eight treatments, and forty-nine for error. The treatments will be further divided into :—

(1)	Nitrogen against No Nitrogen	1	degree.
(2)	Potash against No Potash	1	,,
(3)	Phosphate against No Phosphate ...	1	,,
(4)	The interaction of Nitrogen and Potash ...	1	,,
(5)	,, ,, ,, Nitrogen and Phosphate	1	,,
(6)	,, ,, ,, Potash and Phosphate	1	,,
(7)	,, ,, ,, Nitrogen, Potash and Phosphate ...	1	,,
	Total	7	

The variance appropriate to each of these component parts will be calculated in much the same way as in the two factor trial. Thus firstly the two factors nitrogen and potash may be considered. There will be in each block two plots which receive no Nitrogen and no Potash (O and P), two which get Nitrogen and no Potash (N and NP), two which get Nitrogen and Potash (NK and NKP) and two which get no Nitrogen and Potash (K and KP). From these four pairs of treatments the sum of squared deviations appropriate to the two factors can be calculated and this split up into that part which is appropriate to the Nitrogen against no Nitrogen comparison, that part due to Potash versus no Potash, and the remainder, which will be allotted to the interaction of the two factors.

In the same way, the factors nitrogen and phosphate, and potash and phosphate will be considered and thus the first six items in the treatment analysis obtained.

The seventh item, the interaction of nitrogen, potash and phosphate, can then be obtained by subtracting the sum of these six from the total sum of squared deviations due to treatment, calculated from the eight differently treated sets of plots in the ordinary way.

ACKNOWLEDGMENTS.

In a paper such as this, which is the fruit of so many workers' experience, merely collected together by the present writer, it is very difficult to mention personally all those to whom he is indebted.

A large part of this paper is the result of fifteen years' experience in horticultural experimentation at the East Malling Research Station, and the writer can claim but a small share of credit for this work.

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With regard to the bibliography the writer has tried to collect as complete a list of papers as possible dealing with the subject ; this could not have been done without the assistance of the officers of the Bureau. For any omissons on this list he would apologize, while he hopes that he has not misrepresented the opinions of those authors, whom he has actually quoted.

APPENDIX " A."

Standard Error of the Coefficient of Variability.

Coefficient of Variability (v)	Number of Observations (n)						
	8	10	20	50	100	200	500
10	2.4	2.1	1.6	1.0	0.7	0.5	0.1
15	3.6	3.3	2.4	1 5	1.1	0.8	0.2
20	5.0	4.5	3.2	2.1	1.5	1.0	0.7
25	6.5	5.8	4.1	2.6	1.9	1.3	0.8
30	8.1	7.2	5.1	3.3	2.3	1.6	1.0
35	9.9	8.8	6.2	3.9	2.8	2.0	1.2
40	11.9	10.6	7.4	4.6	3.3	2.3	1.5
45	14.2	12.6	8.7	5.4	3.8	2.7	1.7
50	16.9	14.8	10.1	6.2	4.4	3.1	1.9

APPENDIX " B."

The Effect of the Coefficient of Variability on the Number of Observations necessary to prove certain differences between two means.

Difference per cent.	Coefficient of Variability (V).								
	10	15	20	25	30	35	40	45	50
10	9	18	32	50	72	98	128	165	200
20	4	6	8	12	18	25	32	41	50
30	3	4	5	6	8	11	14	18	22
40	2	3	4	4	5	6	8	10	13
50	2	2	3	3	4	4	5	7	8

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